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NOTES ON THE POTTERY OF PECOS

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THE pueblo of Pecos in San Miguel county, New Mexico, was abandoned in 1838; after a presumably continuous occupation from an early period in prehistoric times. Its rubbish heaps, therefore, contain the accumulations of several centuries, and some of them are so stratified that by a careful study of their contents one is enabled to trace the development of the arts and industries of the community from beginning to end. Of the various objects found in these stratified heaps, pottery fragments are by far the commonest and most easily classifiable, and it is with them that the present paper is concerned.

That there have been changes in the pottery of the Rio Grande pueblos in prehistoric and historic times, has been recognized by archaeologists for a number of years, but no sure evidence as to the sequential order of the several recognizable styles was forthcoming until Mr. Nelson found at San Cristobal a stratified rubbish heap containing wares of three of the principal earlier types. Similar material from other sites enabled him to place chronologically two later types that brought the sequence unbroken down to a comparatively recent date (*ca.* 1700).[1] These investigations served, in the words of Dr. Kroeber, to convert "the archaeological problem of the Southwest from an essentially exploratory and descriptive

[1] N. C. Nelson, "Chronology of the Tano Ruins, New Mexico," *American Anthropologist* (N.S.), vol. 18, pp. 159–180. Here referred to as "*N.*"

one, with interpretation based chiefly on Spanish documentary and native legendary sources, into a self-contained historic one." Mr. Nelson's discovery, and his discriminating analysis of his material constitute, therefore, one of the most important contributions that has yet been made to North American archaeology.

As Pecos was inhabited for a longer time than any other historic Southwestern site available for excavation;[1] and as its compact form, imposed by its position on a small mesa, tended to confine its rubbish heaps to a small area, an almost complete ceramic series can there be recovered in exact order. Thus we are able to cross-reference and, in some respects, to supplement the data secured by Mr. Nelson. We publish our results at this time, some years before the final report on the complete investigation can be brought out, and in their present fragmentary form, in order that they may become available for the constantly increasing number of archaeologists working in the Southwest.

The paper is in three parts: first, a classification of the wares found at Pecos, with condensed descriptions of the different types; second, the presentation and analysis of the stratigraphical evidence which has served to place them in their proper chronological order; third, a discussion of the sequence of the types.

Classification and Description

In making this study we have used only sherds, the complete vessels found in the excavations not yet having been restored; hence we are unable to give any data on the shapes of whole pieces, or to include more than the most general notes on decoration. As it is difficult to describe in words such subtle qualities as the differences between nearly related groups of pottery, we have picked out a short series of sherds illustrating all the types. This series will be sent (on application to the Department of Archaeology, Phillips Academy, Andover, Mass.) to any one who cares to use it, either for a clearer understanding of the present paper, or for comparison with collections from other parts of the Southwest.

[1] Acoma is the only pueblo which has not changed its location at least once since 1540, and as to the length of its prehistoric occupation we are ignorant; it is possible, therefore, that Pecos was absolutely the longest inhabited village in Arizona or New Mexico.

The classification follows:

I. DULL-PAINT WARE.
1. Black-on-white.
2. Biscuit.
A. Rough exterior.
B. Smooth exterior.
3. Modern.

II. GLAZE-PAINT WARE.
1. Two-color.
A. Black-on-red.
B. Black-on-yellow.
2. Transitional.
3. Early three-color.
4. Pajaritan three-color.
5. Late three-color.
6. Degenerate.

III. UNDECORATED SMOOTH WARE.
1. Polished black.
2. Plain red.

IV. BLACKWARE.
1. Corrugated.
2. Strong blind-corrugated.
3. Medium blind-corrugated.
4. Faint blind-corrugated.
5. Featureless.
6. Striated.

I. DULL-PAINT WARES

1. *Black-on-white*

Rim-shape.—Bowls; sides rise rather straight with no thickening at rim. Lip usually flat, but with rounded corners (pl. 1, fig. 1). Rounded and thin rims (figs. 2 and 3) present but uncommon. About 10 per cent. of bowl-sherds show outcurved rims with rounded lips (fig. 4). Ollas; insufficient data.

Surface-finish.—Slip-color ranges from light to dark gray, very rarely pure white; it is applied to the interior of bowls in a relatively heavy coat; to the exterior usually as a thin wash, occasionally as a heavy coat, rarely omitted altogether. Slip often crackled, particularly when it is applied heavily on the exterior.

Finish of interior even but never glossy; of exterior much rougher. No specimen with corrugated or basket-marked exterior observed.

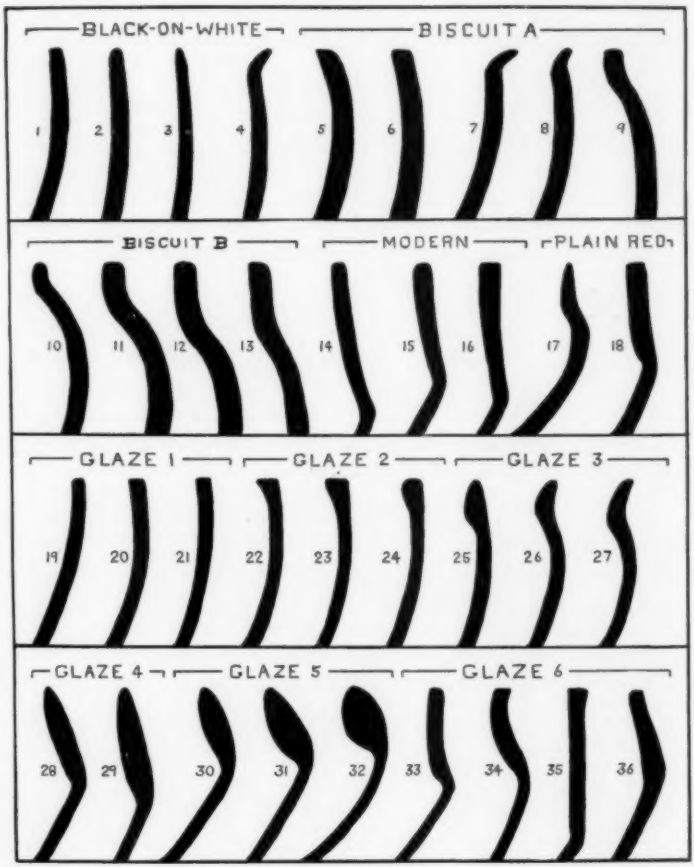
Ornamentation.—Pigment dull black, usually of a slaty cast. Zones: interiors of bowls, exteriors of ollas; exterior decoration of bowls exceedingly rare. Straight bowl-rims usually plain, occasionally dotted; outcurved rims bear ticks or, more commonly, a single heavy zigzag line. The main ornament appears to have been in the form of a broad band encircling the interior of the bowl and leaving only a small blank space in the bottom. Bands framed above and below by single heavy lines ("*N*," pl. VII, fig. 7); less commonly by one heavy line with a series of lighter ones between it and the band ("*N*," pl. VII, fig. 8). Line-break in framers rare. "All-over" decorations apparently fairly common, particularly on small bowls. Design preponderatingly geometric and rectilinear, life and curvilinear forms practically absent. Elements of design most commonly observed: coarse hatching and cross-hatching; plain and dotted checkerboard; series of plain triangular figures; dotted lines and edges; large stepped figures in opposed pairs. Brushwork normally crude and uncertain, lines coarse.

Paste Composition.—Paste fine, very hard and homogeneous; color ranges from pure white (rare) through light gray to dark gray (rare). Many sherds are of even color from surface to surface, but in the majority the center is darker than the edges. Gross tempering (sand or pounded rock) apparently absent; most sherds show a sparse sprinkling of light and dark flecks (nature undetermined). Average thickness of ware for bowls slightly over $\frac{3}{16}$ inch; for ollas about $\frac{4}{16}$ inch.

2. *Biscuit-ware*

Biscuit-ware is a soft, thick, and porous pottery with light colored slip and dull black paint. It is confined pretty strictly to the Rio Grande drainage, where it is most common in the northern districts: Taos, Abiquiu, the Chama, the Oso basin, etc. In the Pajarito country its abundance relative to other wares varies at different sites;[1] at Pecos and in the Galisteo basin it is less frequent than in the Pajarito and Keres regions ("*N*," p. 169); in the Manzanos and along the Rio Grande in southern New Mexico it is apparently rare. Its chronological position and morphological relationships will be discussed below. The division of the ware into two groups (*A* and *B*) is based on technological and decorative features which are believed to represent more than mere variation within a single group. Type *A* comprises all pieces which have

[1] See Kidder, "Pottery of the Pajarito Plateau and of some Adjacent Regions in New Mexico," *Memoirs American Anthropological Association*, vol. II, part 6, p. 459. Here referred to as "*K*."



BOWL-RIM SECTIONS

unslipped rough exteriors; Type *B* those with slipped and decorated exteriors. Further differences between the two will be brought out in the following descriptions:

Biscuit A

This particular variety has hitherto been reported from only one site, the Agua Fria "Schoolhouse" ruin ("*K*," p. 449); its presence in the Galisteo basin is to be inferred from Nelson's description of biscuit-ware with "decoration on one side" (p. 169).

Rim-shape.—Bowls (ollas not identified): there are three types; the plain (pl. 1, figs. 5, 6), the outcurved (figs. 7, 8), and the standing (fig. 9); the outcurved is much the most common. In all three the bowl-sides are more strongly curved than in black-on-white bowls (*cf.* figs. 1 to 4). Plain type: the lip is essentially flat and horizontal; the exterior edge of the flat top is rounded, the interior sharp (fig. 5). Outcurved type: the lip is somewhat thinner than the side of the vessel and has a rounded termination; the width of the flare, its angle, and the extent of its thinning, vary considerably (figs. 7, 8). Standing type: the rim proper is vertical; the lip is flat-topped with rounded edges.

Surface-finish.—Slip a very even, dull light-gray (very seldom if ever yellowish); well smoothed but seldom "slick" and never polished; rarely crackled; is applied to interior only. The unslipped exterior has a very characteristic texture, showing the close-set and irregularly horizontal striations of a scraping tool; the contours, however, are even and true, much more so than the rather irregular exteriors of black-on-white bowls; color dark gray.

Ornamentation.—On interior only. Pigment a dull, deep black, lacking the bluish cast of black-on-white. The lips of some of the straight-rimmed and most of the standing pieces are dotted; the inner surfaces of the standing examples and the flaring part of the outcurved ones are decorated with: a single zigzag line; or a series of dextrally oblique dashes; or with small, elongated rectangles containing one, two, or three parallel horizontal lines. The main design is apparently almost always of band type, leaving a rather large clear space in the bowl-bottom. The bands are usually framed above and below by single very heavy lines, less commonly by narrower single lines with interior sets of minor framers. Line-break not noted. Elements of design (such as key-figure, hatching, cross-hatching, and checkerboard) seem to be closer to the black-on-white system than to biscuit *B*, though a few motifs characteristic of the latter (awanyu and standard triangle—see "*K*," pl. XVIII, and fig. 13) seem to be present in an undeveloped form. Brushwork averages coarser and more irregular than in black-on-white.

Paste Composition.—Less compact, softer and more porous than black-on-white; fractures more irregularly; has a characteristic "woodeny" feel, and lack of ring when struck. Where the color is not uniform from surface to surface

the pieces always become progressively darker toward the exterior. No gross tempering at all; a few white flecks through the paste. Average thickness of bowl $\frac{4}{16}$ inch.

Biscuit B

This ware is characterized by the presence of slip and decoration on both interior and exterior of bowls, and by high, square-lipped rims; also by variability of slip-color, which ranges from almost white to almost yellow. Nelson (p. 169) divides the ware into two classes: white and yellow, but we believe these to be merely variants (due to the presence of oxides in the paste, and to differences in firing) from a light gray normal. Biscuit *B* from the Pajarito, which seems much like that of Pecos, is described at some length in "*K*" *passim*.

Rim-shape.—Bowls (ollas not yet distinguishable from biscuit *A*, so are here disregarded): standing rim practically the only type, flaring or unmodified examples being rare. The upper wall of the bowl curves inward and the rim rises more or less vertically without any thickening. Lip flat and horizontal with rounded corners. The rim cross-sections given (pl. 1, figs. 10, 11, 12, 13) are believed to represent a development from early to late.

Surface-finish.—Slip applied to exteriors as well as interiors; usually dull light gray, some pieces fired (or smoked in use?) to dark gray, others of a yellowish cast. Surface smooth, but seldom "slick" and never polished. Slip rarely crackled.

Ornamentation.—In dull black pigment like that of type *A*. The lips of bowls regularly are ticked at about ½-inch intervals with large dots of paint. Inner surfaces of rims bear zigzags, festooned lines, vertical or oblique dashes. The body designs are generally of band type both interior and exterior; framed above and below by single heavy lines with series of thinner framers between them and the decoration proper. The design system is comparable to that of Pajaritan biscuit (see "*K*" *passim*) the triangle, awanyu, double flag, cut-down rectangle, and hatched triangle, all appear. Zigzag decorations seem rare, nor have we noticed the cross-bird. The line break is uncommon or perhaps absent.

Paste Composition.—Not distinguishable from biscuit *A* in consistency, tempering or thickness; but pieces of yellowish shade are commoner and average brighter yellow.

3. *Modern Painted*

This ware bears so strong a family resemblance to the present-day decorated pottery of certain of the eastern pueblos, *viz.*, Santo Domingo, Cochiti, San Ildefonso and Tesuque, that did stratigraphical evidence not show it to be of late date its modernity

could safely be inferred on comparative grounds. The question of the ancestry of modern ware is discussed in the concluding section.

Rim-shape.—Bowls: very characteristic, high (from ¾ to 3 inches, average about 1¾ inches), and gently curved (see pl. 1, figs. 14, 15, 16): lip flat with rounded edges. Small, shallow bowls with plain rims present but rare. Ollas: little data, but apparently always flaring with round lip (*cf.* pl. II, fig. 6).

Surface-finish.—Slip in two colors: red and yellowish-buff.[1] The former is a heavy soft coat, susceptible to wear and to chipping; it is very commonly crackled; usually well finished, often "slick," rarely glossy. The red slip is brighter than the reds of glazed ware, is softer and is brought to a *well-polished* surface. For range of the slips see under ornamentation.

Ornamentation.—In dull black pigment not distinguishable from that of biscuit, red paint used for fillings in a few cases. Zones: on bowls usually only the exterior of the rim, that surface being covered (from outer edge of lip down to the shoulder) with light slip to receive the design; in these cases the light slip is underlaid by the red slip which extends from 1 or 2 inches below the shoulder over the lip to (but seldom beyond) its inner edge. The whole interior of the bowl and its exterior bottom are left in the paste, which is only fairly well smoothed. A few bowls are decorated in the interior (as well as on the exterior) in which cases the light slip also extends over that surface. Ollas: little data as to zones; some apparently decorated in black over light slip from shoulder to lip; others in a narrow band about, or just above, shoulder. In all cases the polished red slip extends from well below the shoulder to well inside the rim, underlying whatever application there may be of the light slip. Design of exterior of bowl rims (insufficient data for ollas and bowl interiors) consists of a band, always framed below, and usually also above, by a single narrow line. Panelling and the repetition of a single element are characteristic. Terraced figures, cloud-festoons, birds (?) and what appear to be vegetal forms are common. Lines are thin and brushwork is somewhat more confident than in biscuit *B*.

Paste Composition.—Harder and less porous than biscuit but, like biscuit, light and "woodeny." Color occasionally dull orange throughout but most commonly very dark gray (almost black) in a central streak which changes to yellow and finally to orange toward the two surfaces. The yellow-orange surface streaks vary in thickness according, apparently, to the intensity of the firing. In most sherds they occur under both surfaces, but are usually stronger under the outer; in some cases they are present only below the outer one. Tempering of sand or broken rock so rare as to seem accidental where present; most pieces show a few small, pure-white flecks which are more striking in this ware than are the corresponding light flecks in the biscuit and black-on-white because of the

[1] Occasionally, though very rarely, pinkish as in the Tano specimens; "*N*," p. 177.

darker shades of the paste. Thickness of ware for bowls just less than ¼ inch; for ollas about $\frac{3}{16}$ inch.

II. GLAZE-PAINT WARES[1]

1. *Two-color*

This class is identical with Agua Fria "Schoolhouse" pottery[2] and with Mr. Nelson's chronological Type II.[3] It is characterized by decoration in thin black glaze applied over red or light colored slips. The red and the light varieties are considered as color phases of a single type because they are found together, are practically identical in vessel-forms, and are very similar in decoration. Mr. Nelson has shown that the red began to be made somewhat earlier than did the light-colored, and our own stratigraphical studies tend to confirm this.

Rim-shape.—Bowls: the general trend of the sides indicates a somewhat more open vessel than in black-on-white or biscuit. The rim is invariably unthickened and plain, the lip tending to squareness, particularly in the yellow examples (pl. 1, figs. 19, 20). The modeling of pieces is very precise and even, the least asymmetry in thickness or in line being unusual. Ollas: rim always straight, with no thickening or flare at the lip (the outline given in plate 2, fig. 1, is characteristic).

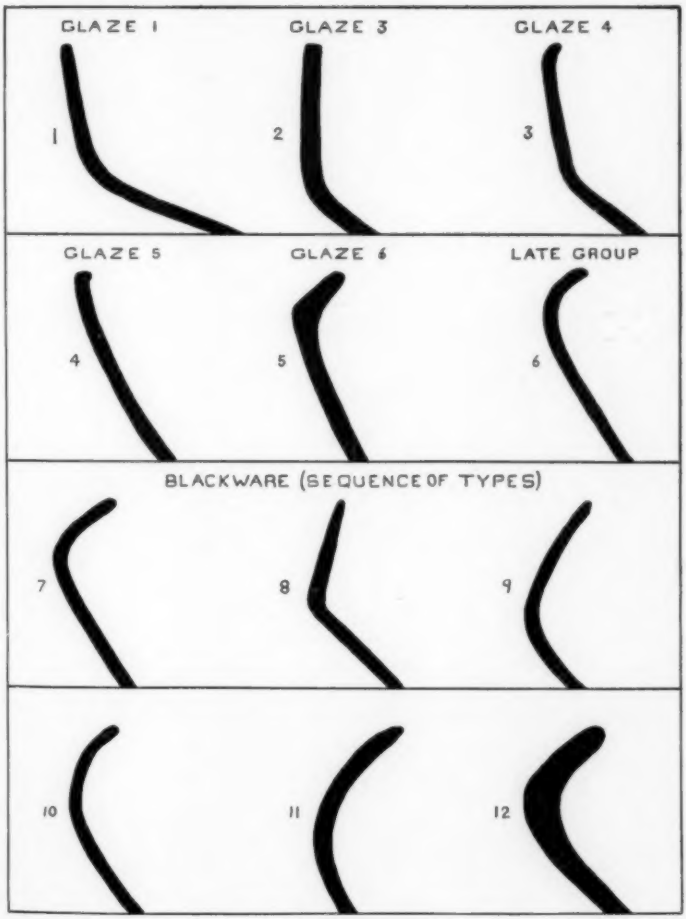
Surface Finish.—Slip; a relatively heavy coat; either a deep rich red; or a light color, varying from light gray to clear lemon-yellow. It seems to cover all surfaces of bowls and whole exterior of ollas. Excellently smoothed, but not polished; no crackling.

Ornamentation.—Glaze-paint. On light colored pieces the glaze is usually thin and "varnishy"; holds well to its lines with little or no running. The most brilliant, lustrous, and blackest glaze occurs on light gray or whitish pieces; on yellower surfaces it is browner, less lustrous and more "varnishy." The glaze, when applied over the red slip, is black, except where thin (then brownish); when extra thick it tends to draw together in little flat "beads," which are very characteristic. Zones of decoration: bowls, interior only (a few oblique dashes or crosses are the sole form of exterior ornament); ollas, upper body from shoulder to base of rim, latter undecorated. Arrangement: a single broad, horizontal band for ollas; the same for all (?) red and some light bowls; many light bowls have designs in the form of long, thin, intensively ornamented triangles which are set horizontally just below the rim (for a good illustration see "*N*," pl. VII, fig. 5). Elements: geometric and strictly rectilinear; opposed key figures,

[1] For discussions of the distribution of these wares see "*N*," p. 160; "*K*," pp. 453 ff.

[2] "*K*," p. 447.

[3] "*N*," p. 172.



OLLA-RIM SECTIONS

hatching, stepped lines, dotted checkerboard, series of small right triangles depending from lines (*cf.* Nelson's description, p. 173). Brushwork even and confident; lines narrow and true.

Paste Composition.—Hard and ringing when struck. Color very variable: gray, yellowish-gray, yellow or even orange; some pieces have a gray central streak with yellow-orange layers under one or both surfaces. No gross tempering, fine white flecks sown thicker than in black-on-white or biscuit. Thickness of ware: bowls $\frac{3}{16}$ inch on the average; ollas a trifle less.

2. *Transitional*

In our preliminary classification, this type was set up as a sort of catch-all for pieces that did not accord well with the standards which were being used for the separation of Types 1 and 3. When these pieces were examined together it was seen that, with some exceptions, they formed not merely a collection of unclassifiable examples or chance variants from the normal of the two types, but a true class technically intermediate between them. In many ways the ware is very similar to "Frijolito" pottery ("*K*," p. 451).

Rim-shape.—Bowls (ollas not as yet surely recognized): even sides, that is with only one curve for each surface, but with more or less strongly thickened rims. The thickening is always on the inside and is always greatest at the edge of the lip; hence the lip itself is flat; it is also horizontal (see pl. 1, figs. 22, 23, 24).

Surface-finish.—Slip usually yellowish or pinkish-gray; red slip rare. Is of poorer quality than Type 1, tending to rub and wear off, showing the reddish paste. The light slip covers whole interior of bowls, and exterior except for the bottom, which is red. Surfaces less smooth than in Type 1.

Ornamentation.—Glaze thin, black, and not often "runny." Is seldom highly lustrous, and has often failed to "take" at all, being merely dull black. Zones: the interior *and exterior* of bowls. Interior decoration always apparently of band type, framed above and below by single broad lines; elements somewhat simplified in comparison to Type 1 (see illustrations of pottery of a very similar nature in "*K*," pl. XXVIII, fig. 4). Exterior decoration in the form of crosses, oblique marks, or red-filled double-ended key figures.

Paste Composition.—Not noticeably different from that of Type 1, except in greater thickness of the ware.

Summary.—Type 2 resembles Type 1 and differs from 3 in: shape (even sides); yellower average color of slip; quality of glaze. It differs from 1 and resembles 3 in: outside decoration; red-filled designs. It resembles neither in: thickened but flat-topped bowl rims. Its transitional nature is most clearly seen in the interior decoration, which lies midway between 1 and 3; in the simplification of design and in the coarsening of the brushwork.

3. *Early Three-color*

Although many pieces of the preceding type show three-color decoration, the practice of using red fillings between glaze lines over a light slip does not become firmly established until we reach the present type. This ware, with our Types 4 and 5, makes up Mr. Nelson's Type III ("Three-color glazed ware;" "*N*," p. 173); those pieces of our Type 2 which have three-color decoration he places in his Type III; those without three colors he assigns to his Type II (see fourth bowl-rim from the left under Type II in his fig. 21).[1]

Rim-shape.—Bowls: almost infinitely variable in minor details of curve and proportion, but always thickened *on the interior a little below the lip*, and almost always having on the exterior opposite the thickening a more or less pronounced outcurve (pl. I, figs. 26, 27). A very few pieces have evenly curved exteriors (fig. 25). Ollas: the rim rises almost vertically (pl. II, fig. 2) or with a very slight inward trend; there is seldom any thickening or modification of the lip.

Surface-finish.—Slip of yellowish or pinkish-gray but of more pallid shades than in Type 2. Many pieces are of a bluish or slaty gray-white; this is evidently due to overfiring and is most commonly seen on exteriors of bowls near their rims; fire clouds common. Slip does not crackle, but wears and chips rather badly. Slip covers whole of bowls except exterior bottom and whole exterior of ollas from the inside of the spring of the neck to just below shoulder; the bottom being red. Finish markedly inferior to Type 1, almost never "slick."

Ornamentation.—Glaze averages brown in color (green and black variants) is heavy and shows some tendency to run. It is usually highly lustrous, but the luster is uneven, being finest where the glaze is heaviest and dying down to a "varnishy" appearance where it is thin. Little blow-holes (due to gas bubbles probably) appear in some heavily glazed lines and are yellow or greeny brown. Lines of decoration heavy and no fine-line work occurs. Ollas have red-filled ornaments about the neck; plain ornament on body from base of neck to shoulder. Bowls: red-filled exterior decoration, plain ornament in interior; the inner surface of the flare of the rim is very often marked with a single heavy zigzag line, less commonly with oblique dashes or parts of zigzags. The interior decorations of bowls and the body designs of ollas are usually horizontal bands, framed above and below with pairs of heavy lines (not red-filled). The bands are panelled by upright lines, and the resultant spaces filled with oblique elements of simple nature.

[1] Reliance upon a single classificational criterion has led Mr. Nelson to misplace one of the ollas in his chronological series (pl. VII). The piece in question is two-color, and he has accordingly grouped it with Type II; its rim-shape and decoration, however, show that it really belongs in his Type III and our Type 4. It is, as a matter of fact, a later piece even than his Type III example.

The "Pajaritan bird" is absent. The exteriors of bowls are always decorated with some form of red-filled key or bird figure.

Paste Composition.—The body of the ware is compact and hard. There is a central gray streak becoming brown or reddish-yellow toward the two surfaces. Most sherds show considerable gross tempering in the form of crushed basaltic (?) rock. Thickness of bowl-walls averages $\frac{3}{16}$ inch, ollas a trifle less.

4. *Pajaritan Three-color*

As its name implies, this ware is similar to the commonest glazed pottery of the Pajarito plateau (that found at Tyuonyi, Tschirege, Otowi, Tsankawi, Puyé).[1] At Pecos it is not very abundant, and its exact place in the ceramic series is not yet clear. It evidently occurs in the Galisteo basin, a typical olla being figured by Mr. Nelson (pl. VII, fig. 6).

Rim-shape.—Bowls: very high, thickened, and with rounded lip; exterior of rim generally flattish (pl. I, figs. 28, 29), rarely a trifle concave; inner side always has a long, even, convex curve. Ollas: rim straight but inclines a little inwards, lip usually recurved outwards (pl. II, fig. 3).

Surface-finish.—Slip in varying shades of light red (very rarely pinkish white), covers all surfaces of bowls, and exteriors of ollas from bottom of interior of rim to well below shoulder, perhaps also bottom. Is carefully smoothed, but seldom "slick" and never glossy.

Ornamentation.—Pigment is black to brown glaze with a high average lustre and fair tractability (not much running). Fillings of many designs are in a red paint much darker in color than the slip. Zones of decoration: interiors of bowls, from base of standing rim downwards; exteriors, from just below rim-edge to about one inch below the under-curve of the bowl-body. Ollas: the exterior of the neck and the upper-body, the two usually being treated as distinct zones. Interior of bowl-rim generally plain, sometimes bears a zigzag line as in Type 3. Bowl-design: interiors have bands or, less commonly, "all-over" patterns. The former are framed with two lines above and two below, each set red-filled. The figures in the bands and in the "all-over" decorations are also often red-filled. Bands are regularly panelled by sets of vertical bars, the panels bearing alternating elements, usually birds and oblique figures (see "*K*," pls. XXII–XXIV). Exterior decoration generally in the form of a double line filled with red drawn about bowl below standing rim; from it rise at intervals, birds or other elements, often red-filled. Independent repeated units (*i. e.*, not attached to a line), such as double-ended keys and three-finger designs ("*K*," pl. XXV and fig. 19) are only fairly common. Olla-design: as a rule the neck decoration follows that of the bowl-exterior; body decoration that of the bowl-interior.

[1] Described with illustrations in "*K*," pp. 417–447.

Paste Composition.—Not distinguishable from that of Type 3 in color, but some examples have considerable gross tempering in the form of smooth-edged bits of quartz (water-worn ?). Average thickness of bowl-walls $\frac{3}{16}$ inch, of ollas a very little less.

5. *Late Three-color*

This is by far the commonest single type found at Pecos; it never occurs, as far as we know, in the Pajarito ruins; the heavy bowl-rim illustrated by Mr. Nelson among his Type III examples (third from the left, fig. 21) seems to show that it is present in the Galisteo basin.

Rim-shape.—Bowls: low, very thick, and strongly incurved; interior of rim more or less convex, according to amount of thickening; lip rounded (see figs. 30, 31, 32). Ollas: the rim continues the curve of the vessel-body without abrupt change of angle, the lip is flat-topped, with a slight flare (fig. 4, pl. 2). Olla-bowls: these are bowls with such restricted apertures as to approach the olla form (see "*K*," pl. XVII, fig. 3; and "*N*," fig. 21, second from the right, top row). At Pecos this form seems to be present only in the Type 5 glazes. In shape of rim the examples are closely similar to the one figured by Mr. Nelson.

Surface-finish.—Slip-color dirty white (commonest), through cream to yellow; rarely red. Slip covers the whole interior of bowls, and exterior nearly to bottom (which is either unslipped, or painted with a thin red wash); ollas, light slip from inside of lip to just below shoulder; bottom, and inside of rim, red. The light slip is a heavy coat, the thickness of writing paper; it is soft, and wears badly; crackles only in heavily overfired pieces. Finish averages poor.

Ornamentation.—The pigment is a very heavy glaze, which varies from dull and "crusty," to a fine lustrous brown with iridescent lights; where it runs out thin it is light-green to canary-yellow. The drips and runs of the glaze are always toward the rim. Red fillings are in a thick, soft pigment which frequently flakes off. Decorative zones: bowls; interior, and upper part of exterior; ollas, neck and upper body. Bowl-design on exterior consists of a red-filled double line encircling the vessel a little below the rim, with crudely drawn red-filled figures, usually birds, perched upon it; a variant of the three-finger decoration ("*K*," fig. 19), made to imitate the human face, is common; double-ended keys are apparently absent. The inner decoration of bowls is seldom more than a pair of red-filled lines running about the pot just below the rim-swelling. Ollas: data insufficient, consists apparently of simple red-filled bands and very crude bird figures. All lines are heavy and carelessly drawn, the potters evidently having realized that the glaze would run in firing and spoil any attempt at fine work.

Paste Composition.—Color: those pieces which have the clearest yellow slip and the most lustrous glaze, are light gray clear through; those with poor glaze and slip are browny-gray in the center and reddish toward the edges. The paste is granular and "sandy," rather thickly tempered with small, dull-white masses

(ground-up sherds?); there are sometimes a few grains of quartz. Average thickness for both bowl- and olla-walls, a little less than ¼ inch.

6. *Degenerate Glaze*

This type corresponds to Mr. Nelson's Type IV. As he points out (p. 175), it is of historic date, and is characterized by diversity of form and by execrable decoration. The ware has so far proved to be much less common at Pecos than at San Cristobal and San Marcos, and to be represented by a smaller range of forms, nothing but bowls and ollas having been found. Our excavations, however, have not yet penetrated to those parts of the site which contain the largest amounts of degenerate glaze.

Rim-shape.—Bowls: extremely varied, but in general there is little thickening (as compared to Type 5), the rims are high, usually nearly vertical, and have a tendency toward reverse curves. A series of four rim-sections is given (pl. I, figs. 33–36); these represent the commonest forms, but no two bowl-rims are ever quite identical. Ollas: the neck continues the slope of the body without abrupt change of angle, the rim is thickened, and the lip is strongly outcurved (pl. II, fig. 5).

Surface-finish.—Slip usually light colored, less commonly red. The light slip is very variable in shade: gray, greeny-gray, pinkish, cream, yellow; it covers the whole interior of bowls, and the exterior nearly to the bottom, which is painted red. Ollas: light-slipped from just within the rim to a little below the point of greatest diameter, bottoms red. Finish generally poor, irregular and "scrapy."

Ornamentation.—The glaze is thick, heavy, and extremely "runny." In color it is usually dark green or dark brown, but where thin is often light green or even yellow. Many examples are dull and poor; where the glaze has "taken" well, however, it is beautifully lustrous. All attempt at precise design is abandoned, the decoration consisting of crude zigzags, blobs, dashes, etc., applied to the exteriors and interiors of bowls and the upper parts of ollas (see "*N*," pl. VII, figs. 1 and 2). There are no red fillings.

Paste Composition.—Variable. Color generally dark gray throughout. Some pieces are very heavily tempered with bits of crushed quartz, others have no tempering beyond a few light-colored flecks. Vessel-walls average thinner than in Type 5.

III. UNDECORATED SMOOTH WARE

1. *Polished Black*

Pottery with high-polished, lustrous black surfaces is found, in the Southwest, on the lower Gila (Arizona), in the Casas Grandes district (Chihuahua), in the Socorro region of New Mexico, and at

Pecos; it is also made today at the pueblo of Santa Clara. The significance of this peculiar and seemingly patchy distribution has yet to be worked out. At Pecos the ware is surely late and is probably all of historic date.

Rim-shape.—Ollas: (bowls very rare) the rim flares out strongly and has a rounded lip (pl. 2, fig. 6).

Surface-finish.—Slip deep black in most cases, occasionally slate-gray; covers whole exterior and runs down about an inch inside the rim. The characteristic polishing of the surface extends from within the rim to well below the shoulder (possibly also over under-body and bottom); the average pieces are only fairly well polished, the best have a perfectly even texture and high sheen.

Paste Composition.—Color deep chocolate brown to almost jet black; thickly tempered with very fine particles of what seems to be pounded quartz; this gives the paste a granular or sandy appearance. The ware is a little harder than biscuit, but like biscuit has a "woodeny" feel. Average thickness $\frac{3}{16}$ inch.

2. *Plain Red*

This ware may be distinguished, even in small sherds, from undecorated red fragments of other classes (such as the bottoms of Types 3, 5, and 6 glaze ollas, bits of Type 1 red bowls, etc.) by its highly polished surfaces and by the color and "feel" of its paste.

Rim-shape.—Bowls: much like modern (see pl. 1, fig. 18) for larger specimens; the smaller bowls (fig. 17) have lower rims with sharp or rounded lips. Ollas do not differ in rim-shape from polished black (*cf.* fig. 6, pl. 11).

Surface-finish.—Slip bright red; covers whole interior of bowls, and often the whole exterior as well, the bottoms of some examples are left unslipped. Ollas: the slip runs from within the rim nearly or quite to the bottom. The finish ranges from indifferent to very highly polished, the finest specimens are always ollas.

Paste Composition.—Apparently exactly the same as modern (*q. v.*).

IV. BLACKWARE

The black cooking pottery, as well as the finer, colored ware underwent changes during the occupation of Pecos. These changes, however, were very much less radical than those in the other classes, and took place so slowly that, while there are quite obvious differences between the earliest and the latest sherds, the intermediate stages can only be made out by placing a number of pieces from each stratigraphical layer side by side and considering them, so to

speak, in the mass. By doing this several successive styles may be recognized. There are, however, so many unclassifiable fragments (presumably from the bottoms and lower sides of the ollas) that we have not attempted statistical treatment of the blackware types, but give instead a sort of running account of the development of the ware, beginning with the earliest known style.

Indented Corrugated—This is not found at Pecos proper, but is present at other nearby sites. It is the local representative of the very widespread archaic cooking pottery of the Southwest; pottery made by laying up on itself coil by coil a continuous thin strand of clay, all traces of the coil being obliterated on the interiors of the vessels, but no smoothing at all being done on their exteriors. Thus, in well-made pieces (and the proportion of these is remarkably high) the coil on the exterior is sharp and definite, each turn being clearly distinguishable. Further, the coil was notched or indented at short, regular intervals during its application to the growing vessel, producing a curious, almost textile, appearance.[1] Such is the earliest cooking pottery of which we now have knowledge in the Pecos valley; it does not occur at the main ruin. There, in the lowest levels of the rubbish heaps, we find a coiled ware which lacks the indented marks and is called, accordingly, *plain corrugated*. This is obviously a simplified form of the preceding one; in fact the whole earlier development of blackware at Pecos is a progressive simplification, the visible signs of the structural coil being steadily eliminated. The next stage we have called *blind corrugated;* the vessels of this class still show the horizontal coil-ridges, but the latter have been so smoothed over in finishing that the sharp cracks between them have been obliterated. Three successive subtypes are recognizable: the *strong*, *medium*, and *faint*. In the latter the coil-ridges have been so nearly destroyed that the surface of the ware shows merely a faint rippling, so slight as to be perceptible only when the light falls across it at a very low angle. In *featureless black*, the next type, all trace of corrugation has disappeared, the vessel exteriors having an even though rough texture. The latest blackware so far identified is the *striated;* there is, of course, no sign of the coil, but the surface is scored with series of fine and more or less parallel scratchings, crisscrossing over each other in all directions. These were produced, evidently, by some finishing tool used while the clay was moist. Experiment shows that a corncob with the kernels removed most nearly duplicates these marks.

Blackware may also be classified on the basis of rim-shape; but, as was the case with surface-texture, the principal types merge into each other very slowly. Type *A* (pl. II, fig. 7): sharply flaring, but with a well-rounded curve between body and rim. Type *B* (fig. 8): very straight rim with sharp break in

[1] For a fuller description of the process, with excellent illustrations see Holmes, "Pottery of the Ancient Pueblos," *Fourth Annual Report Bureau of American Ethnology*.

angle from body, less flare than in any other type. Type *C* (fig. 9): rim still straight, but higher, with more flare, and with less abrupt (though still noticeable) break in angle. Type *D* (fig. 10): high rim, evenly curved flare. Type *E* (fig. 11): heavy ware, even curve, wider flare. Type *F* (fig. 12): *thickened* rim, very sharp flare, solid construction.

These six rim-types seem to correspond stratigraphically in a general way to the six surface-texture types (striated with *F*, featureless with *E*, faint blind-corrugated with *D*, etc.). Both series of types, however, are vague at best, so that single black sherds cannot be relied upon as sure evidence for the dating of strata or deposits; but masses of them sufficiently large to enable one to discount minor variations and to judge of their average character, would undoubtedly be of value.

Stratigraphical

Having classified the wares, we must turn to stratigraphy for our determination of their chronological order. Material for this study was gathered by means of test sections similar to those made by Mr. Nelson; columns of rubbish running from surface to hardpan were isolated, and from them was taken their entire pottery content, the fragments from the different levels or cuts being kept separate. The Pecos tests were not divided into exactly equal cuts, as was done by Mr. Nelson, but were laid out in nearly equal divisions based on sand, ash, or other strata which indicated actual levels of deposition during the formation of the mound. This was necessitated by the fact that the Pecos deposits were for the most part laid down on sloping or irregular surfaces, and cuts made on arbitrarily chosen plane levels would have resulted in the splitting or cross-cutting of strata. This method derogates from the absolute statistical value of the material, as the cuts, not being of exactly equal thickness, are not strictly comparable statistically. The cultural results, for which these tests were taken, are, however, satisfactory; and even the statistical outcome seems fairly significant. Columns of debris have been saved intact in all parts of the heap, so that more exactly subdivided sections can be taken if desired.

Of the four tests here considered, three (whole series nos. X, XI, XII) are from the great rubbish heap on the east slope of the Pecos mesa. The earth-column of test X was 20 feet long, 5 feet wide, and 18 feet high; its eight cuts (numbered as in all the others

from top to bottom) averaged 2 feet 3 inches in thickness. Tests XI and XII were each 25 feet long, 5 feet wide, and 12 feet high, XI had seven cuts of about 1 foot 8 inches in thickness; XII, eight cuts of 1 foot 6 inches. These tests were made very large in area in order that we might get a great amount of sherds for the preliminary study, and also to neutralize as far as possible the disturbing effect of graves, from which no part of the eastern heap is entirely free. The fourth test (XIII) was made in a midden below the northwest corner of the main pueblo; it was approximately 3 feet square, by 4 feet high, and had seven 7-inch cuts. This section was gone through with greater care than the other three, even the tiniest sherds being saved.[1]

The material was handled as follows: the sherds from each cut were classified according to ware and to vessel-forms, and the numerical results entered under the headings given in the accompanying tables (nos. I, III, V, VII). Further, the glazed bowl-rim sherds were subjected to closer study (for a purpose which will be made clear below); they were classified, counted, and the results entered in tables II, IV, VI, and VIII. Finally one set of charts was prepared to illustrate graphically the distribution in percentages of the principal wares (*i. e.*, all black, all glaze, all biscuit, etc.); and another set to show in the same way the range of the types of glaze.

Looking at the four ware-charts, we see that each one of them contains two approximately normal frequency curves: (1) for glaze; (2) for biscuit. All show black-on-white very strong at the beginning and declining rapidly as glazed ware rises. Furthermore, each one exhibits the start of three other curves: those for modern painted; plain red; and polished black. The line representing rough black runs more or less irregularly across each chart.

Blackware as a class obviously has no chronological significance. It was evidently a somewhat variable, but nevertheless constantly important factor in the total ceramic output of the pueblo during its whole occupation.[2] As to the meaning of the tendency of the

[1] In X, XI, and XII no fragment less than ¾-inch across was kept; pieces smaller than this can seldom be accurately classified.

[2] We leave out of consideration the sub-types of blackware; these are without doubt chronologically significant, but our data are not yet sufficiently full to allow us to recognize them with certainty.

TABLE I

TEST X

	Number of Sherds								Approximate Percentages							
Cut No.	1	2	3	4	5	6	7	8	1	2	3	4	5	6	7	8
Black olla rim[1]	44	28	36	28	26	10	38	4	9	5	6	3	4	2	6	3
Black olla body	237	236	173	269	151	109	164	29	46	46	29	34	25	25	26	22
Polished black[2]	51	19	2						10	4	×[6]					
Late plain red[3]	87	51							17	10						
Modern painted[3]	14	11	3						3	2	×					
Glaze bowl rim	19	32	93	85	82	42	32	1	4	6	15	11	13	10	5	1
Glaze bowl body	28	56	139	169	129	102	95	2	5	11	23	22	21	23	15	2
Olla-bowl[4]	1	4	2						×	1	×					
Glaze olla rim	2	3	7	12	10	5	6		×	1	1	1	2	1	1	
Glaze olla body	20	62	105	152	126	113	53		4	12	17	18	21	26	8	
Biscuit *B* bowl rim		4	6	10	12	6	5			1	1	1	2	1	1	
Biscuit *B* bowl body	1	4	24	37	34	10	4	1	×	1	4	5	6	2	1	1
Biscuit *A* bowl rim			2	5	9	10	16				×	1	1	2	3	
Biscuit *A* bowl body	4		4	22	24	33	61		1		1	3	4	7	10	
Biscuit olla[5]			8	9	3						1	1	×			
B.-on-W. bowl rim	1		6	4	1	1	47	28	×		1	×	×	×	7	21
B.-on-W. bowl body	6	1	4	4	4	6	104	60	1	×	1	×	1	1	16	46
B.-on-W. olla					2		9	6					×		1	4
Totals	515	511	614	806	613	447	634	131	100	100	100	100	100	100	100	100

[1] Only ollas occur.

[2] Only ollas occur; body and rim sherds listed together.

[3] Bowls and ollas; body and rim sherds listed together.

[4] Glazed ware; body sherds of olla-bowls are not certainly distinguishable from those of undecorated bowl body sherds, hence only rims are listed.

[5] We cannot yet distinguish Biscuit *A* ollas from Biscuit *B* ollas.

[6] Present, but less than one half of one per cent.

TABLE II

OCCURRENCE OF GLAZED WARE TYPES AS SHOWN BY BOWL-RIMS. TEST X.

	Number of Sherds								Percentages							
Cut No.	1	2	3	4	5	6	7	8	1	2	3	4	5	6	7	8
Type 6	4	1	1						.7	.1	.2					
Type 5	10	15	15						1.9	2.9	2.4					
Type 4		6	22							1.1	3.5					
Type 3	2	5	44	32	5	2			.3	.9	7.1	3.9	.8	.4		
Type 2	1	3	8	35	57	2			.1	.5	1.3	4.3	9.5	.4		
Type 1 (yellow)	2	2	2	10	14	28	15		.3	.3	.3	1.2	2.2	6.2	2.3	
Type 1(red)			1	8	6	10	17	1			.2	.9	.9	2.2	2.6	.7
Totals	19	32	93	85	82	42	32	1	3.3	5.8	15.0	10.3	13.4	9.2	4.9	.7
Type I (both phases)	2	2	3	18	20	38	32	1	.3	.3	.5	2.1	3.1	8.4	4.9	.7

TABLE III

TEST XI

	Number of Sherds							Approximate Percentages						
Cut No.	1	2	3	4	5	6	7	1	2	3	4	5	6	7
Black olla rim	59	38	23	16	27	15	4	10	8	6	7	6	4	6
Black olla body	165	116	56	64	92	75	17	29	23	16	30	20	21	27
Polished black	41	15	1					7	3	×				
Late plain red	54	32	1					9	7	×				
Modern painted	21	18	1					4	3	×				
Glaze bowl rim	105	116	92	42	79	61	1	18	23	25	20	17	17	2
Glaze bowl body	68	73	76	50	115	82	2	12	15	21	23	25	23	3
Alla-bowl	7	12	12	4				1	2	4	2			
Glaze olla rim	12	19	19	2	8	9		2	4	5	1	2	3	
Glaze olla body	44	62	83	28	53	50		8	12	23	13	12	14	
Biscuit *B* bowl rim	1		1	5	16	4		×		×	2	3	1	
Biscuit *B* bowl body	1	1		3	46	9		×	×		2	10	2	
Biscuit *A* bowl rim				1	3	13	1				×	1	4	2
Biscuit *A* bowl body					8	27	3					2	8	5
Biscuit olla					6							1		
B.-on-W. bowl rim				1	2	4	7				×	×	1	11
B.-on-W. bowl body				1	6	8	23				×	1	2	37
B.-on-W. olla	1				1	1	4	×				×	×	7
Totals	579	502	365	217	462	358	62	100	100	100	100	100	100	100

TABLE IV

OCCURRENCE OF GLAZED WARE TYPES AS SHOWN BY BOWL-RIMS. TEST XI

	Number of Sherds							Percentages						
Cut No.	1	2	3	4	5	6	7	1	2	3	4	5	6	7
Type 6	16	11	1					2.7	2.1	.2				
Type 5	83	101	74	15	3			14.3	20.1	19.1	6.8	.6		
Type 4	2	3	12	17				.3	.6	3.2	7.8			
Type 3	2	1	2	8	58	5		.3	.1	.4	3.6	12.5	1.5	
Type 2			1	1	10	20				.2	.4	2.1	5.5	
Type 1 (yellow)			1		4	21				.2		.8	5.8	
Type 1 (red)	2		1	1	4	15	1	.3		.2	.4	.8	4.1	1.6
Totals	105	116	92	42	79	61	1	17.9	22.9	23.5	19.0	16.8	16.9	1.6
Type I (both phases)	2		2	1	8	36	2	.3		.4	.4	1.6	9.9	1.6

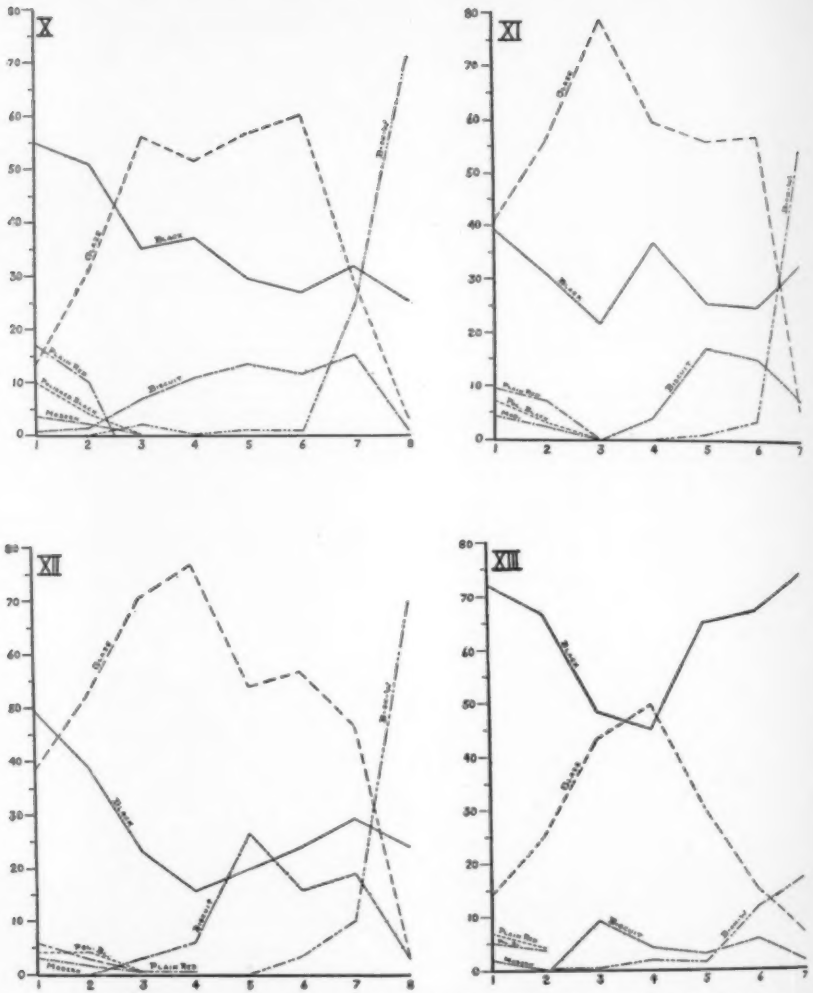


FIG. 54.—Charts illustrating graphically the distribution in percentages of the principal wares.

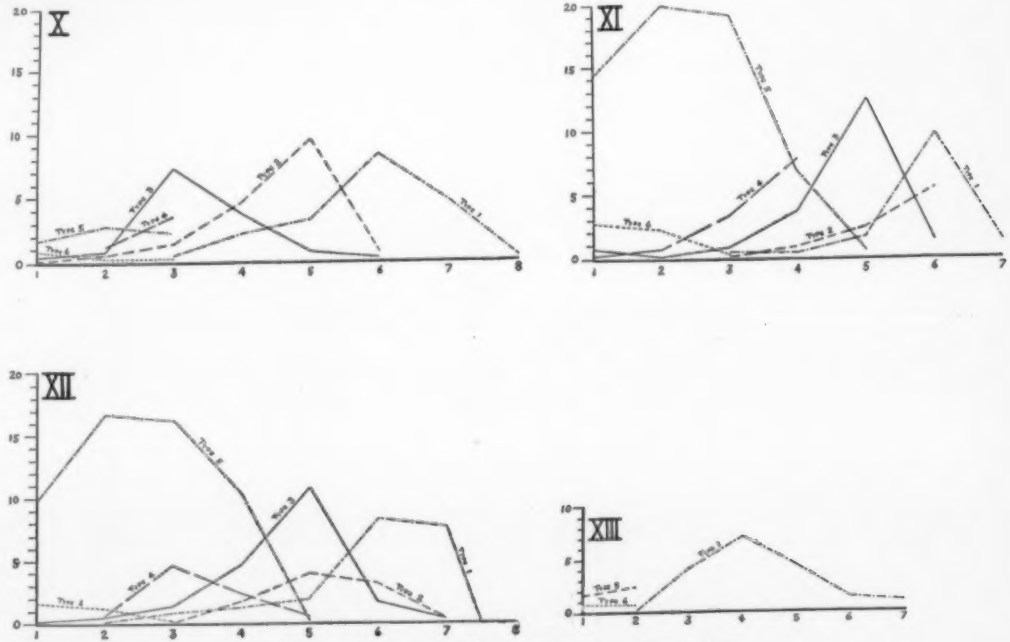


FIG. 55.—Charts illustrating the range of the types of glaze.

TABLE V

TEST XII

	Number of Sherds								Approximate Percentages.							
Cut No.....	1	2	3	4	5	6	7	8	1	2	3	4	5	6	7	8
Black olla rim.....	41	132	32	6	20	31	10	6	11	11	7	4	5	5	4	7
Black olla body....	143	335	75	20	63	128	66	14	38	28	16	12	15	19	25	17
Polished black.....	14	54	3						4	4	1					
Late plain red.....	20	23	4	2					6	3	1	1				
Modern painted....	11	25	4						3	2	1					
Glaze bowl rim.....	43	232	114	34	74	83	23	2	12	19	23	21	18	13	9	2
Glaze bowl body...	53	201	104	55	88	131	49	1	14	17	21	33	21	20	18	1
Olla-bowl.....	2	34	15	3					1	3	3	2				
Glaze olla rim.....	2	20	20	6	10	17	4		1	2	4	4	2	3	2	
Glaze olla body....	35	131	94	29	53	141	35		10	11	20	17	13	21	13	
Biscuit *B* bowl rim .		5	2	1	16	8	1			×	×	1	4	1	×	
Biscuit *B* bowl body	1	1	9	1	65	42	4	1	×	×	2	1	16	6	2	1
Biscuit *A* bowl rim .				2	3	21	13	2				1	1	3	5	2
Biscuit *A* bowl body	1		5	3	8	36	32		×		1	2	2	6	12	
Biscuit olla.....		1		1	12	3				×		1	3	×		
B.-on-W. bowl rim .		1			2	5	9	18		×			×	1	3	21
B.-on-W. bowl body		1			2	13	15	40		×			×	2	6	48
B.-on-W. olla.....							3	1							1	1
Totals.....	366	1,196	481	163	416	659	264	85	100	100	100	100	100	100	100	100

TABLE VI

OCCURRENCE OF GLAZED WARE TYPES AS SHOWN BY BOWL-RIMS. TEST XII

	Number of Sherds								Percentages							
Cut No.....	1	2	3	4	5	6	7	8[1]	1	2	3	4	5	6	7	8
Type 6.....	6	16	1						1.6	1.3	.2					
Type 5.....	36	201	78	17	1				9.8	16.7	16.2	10.4	.2			
Type 4.....		8	23	4	2					.6	4.7	2.4	.4			
Type 3.....	1	6	7	8	46	7	1		.2	.5	1.4	4.8	10.9	1.7	.3	
Type 2.....			1	3	17	21	1				.2	1.8	4.0	3.1	.3	
Type 1 (yellow).....		1	2	2	6	28	7			.1	.4	1.2	1.4	4.2	2.6	
Type 1(red).....			2		2	27	14				.4		.4	4.0	5.3	
Totals.....	43	232	114	34	74	83	23		11.6	19.2	23.5	20.6	17.3	13.0	8.5	
Type I (both phases)		1	4	2	8	55	21			.1	.8	1.2	1.8	8.2	7.9	

[1] The two glazed bowl rims in cut 8 were too battered for identification.

TABLE VII

TEST XIII

	Number of Sherds							Approximate Percentages						
Cut No.	1	2	3	4	5	6	7	1	2	3	4	5	6	7
Black olla rim	26	26	2	6	12	22	16	4	3	1	3	3	4	5
Black olla body	415	472	87	90	254	320	234	68	64	47	42	62	63	69
Polished black	43	33						7	4					
Late plain red	30	29						5	4					
Modern painted	11	4						2	×					
Glaze bowl rim	16	26	8	15	19	8	5	3	3	4	7	5	2	1
Glaze bowl body	45	97	36	45	59	41	16	7	13	19	22	14	8	5
Olla-bowl		2							×					
Glaze olla rim		3	3	2	3				×	1	1	1		
Glaze olla body	25	65	35	39	44	26	4	4	9	19	19	10	5	1
Biscuit *B* bowl rim	1		2					×		1				
Biscuit *B* bowl body	1	1	3	3	1			×	×	2	1	1		
Biscuit *A* bowl rim		3	1	1					×	1	1			
Biscuit *A* bowl body		3	10	4	12	29	9		×	5	2	2	6	2
Biscuit olla														
B.-on-W. bowl rim		1		3	2	5	14		×		1	1	1	4
B.-on-W. bowl body	3	1	1	2	6	51	38	×	×	×	1	1	10	11
B.-on-W. olla				1		4	5				×	×	1	2
Totals	616	766	188	211	412	506	341	100	100	100	100	100	100	100

TABLE VIII

OCCURRENCE OF GLAZED WARE TYPES AS SHOWN BY BOWL-RIMS. TEST XIII.

	Number of Sherds							Percentages						
Cut No.	1	2	3	4	5	6	7	1	2	3	4	5	6	7
Type 6	6	6						.9	.8					
Type 5	10	17						1.7	2.2					
Type 4														
Type 3														
Type 2														
Type 1 (yellow)		2	5	11	8	2			.2	2.6	5.2	1.9	.5	
Type 1 (red)		1	3	4	11	6	5		.1	1.5	1.9	2.6	1.0	1.4
Totals	16	26	8	15	19	8	5	2.6	3.3	4.1	7.1	4.5	1.5	1.4
Type 1 (both phases)		3	8	15	19	8	5		.3	4.1	7.1	4.5	1.5	1.4

blackware curves to dip toward the middle of the charts, and to rise toward their upper ends, we cannot attempt to judge. The total frequency of black relative to other wares seems to depend on the size of the sherds collected. Black vessels break into very many very small fragments, so that if only sherds of 3 inches square were kept, black would fall to almost nothing; whereas if the most minute bits were saved it would rise to well over 65 per cent. An example of this is seen in the plot of test XIII, a section including small fragments.

Black-on-white ware is shown in each chart to drop rapidly from a high initial percentage, its appearance in very small quantities in the middle and upper cuts (as in test X) being due, probably, to churning of the heaps during deposition, or to the dumping of old rubbish from the mesa top. In no case does black-on-white make a respectable showing after the real beginning of the rise of glaze.

Glazed ware is represented by normal frequency curves in all the charts, rising from practically nothing at the bottom, to a high percentage at or somewhat beyond the middle, and declining toward the end little less rapidly than it rose. Although no test is free from glaze in its lowest cuts, nor does any one show the entire extinction of glaze as a ceramic trait, we believe, from evidence other than stratigraphical, that there was a short period at the beginning when the art was not practised, and that no glazed ware was made during the closing years of the occupation of Pecos.

The curves for biscuitware are comparable to those for glaze; both rise together, and both tend to have similar contours. There are, however, these important differences: (1) that biscuit is always a minor or secondary ware in point of frequency; (2) that while glaze runs as far as or even beyond the upper limits of each test, biscuit has a considerably shorter life, its curves becoming negligible in every case while those for glaze are near their highest points.

The remaining three curves: those for polished black, plain red, and modern painted, have their origin at about the point of extinction for biscuit, and are on the rise in the last cut of each test. There is, however, so little overlapping of the biscuit curves

and of these three later ones that we do not believe that the wares actually overlapped in time. It seems more probable that polished black, plain red, and modern painted are all parts of a single ceramic group that replaced the glazed wares.[1] This we may call for convenience the late group.

To sum up: it appears that there were three main stages in the ceramic history of Pecos: the black-on-white, the glazed, and the late; that the black-on-white ended soon after the settlement of the mesa, and the late began only shortly before its abandonment. Within the glazed period, then, falls most of the history of Pecos. For this reason a close study of the glazed ware is essential and is greatly facilitated by the fact that the pottery is subdivisible into six easily recognized types.

A cursory examination of the test material, when sorted and laid out cut by cut, shows that three of these glazed ware types, namely nos. 1, 3, and 5, succeed each other in the order given. The chronological positions of the three others (nos. 2, 4, and 6) are strongly indicated: (1) by their occurrence at certain levels in the tests; (2) by technological and decorative features. One would like, however, to have more tangible evidence as to the exact relations of these latter to the more obviously placeable groups (1, 3, 5) than can be had by a mere inspection of the test material. For this purpose we have plotted the distribution of all six types.[2] In the resultant diagrams each of the plotted types tends to form a normal frequency curve, indicating that each one had a natural rise, vogue, and decline. One's general impression is that they are all successive phases of the single glazed pottery trait, and that each one of them developed from its predecessor.

As to the position of Types 1, 3, and 5 there can be, as was remarked above, no doubt. Type 1 is surely the oldest, it being

[1] Here we seem to get a hint that when a style breaks up it is succeeded by two or more new styles (as black-on-white by glazed and biscuit); and that one of these eventually dominates. More research is needed.

[2] As it is impossible to classify accurately undecorated sherds from glazed vessels, and as there is often uncertainty even in the case of decorated fragments which do not show the rim-contour, we have used only rim-sherds in making the type distribution study. What is lost in bulk of material is more than made up for in certainty of identification.

the only one represented in the bottom cut of each of the tests; in test X, indeed, it is the only type in the two bottom cuts, and it stands by itself in the five lowest cuts of test XIII. In test X it is succeeded quite definitely by Type 2, which in turn reaches its maximum and is well along in decline before the real rise of Type 3. In tests XI and XII the evidence for placing Type 2 between 1 and 3 is somewhat less decisive, and were it not for test X and for the marked transitional appearance of the ware itself, we might perhaps have been led to consider it as a contemporary variant of Type 3.

In the case of Types 4 and 5 we are confronted with rather greater difficulty. Type 5, as is shown by the plots of tests XI and XII, is without question a later style than 3; furthermore it has from the technical and decorative points of view the appearance of being a direct outgrowth from it. Type 4, according to the plots, lies within the limits of distribution of Type 5; in tests X and XI it comes in abruptly; nowhere do we get any direct evidence, as was the case with Type 2 in test X, for calling Type 4 transitional or even chronologically intermediate between 3 and 5. The ware itself does not help us, as it fails to fit well into what seems to be the developmental series. One point, however, should be noted: that the curves for Types 2 and 4 are very similar in test XI; and also very similar (though of different form from those in test XI) in test XII. Type 2 is fairly well established as transitional, so that the resemblance between its curves and those of Type 4 may be significant.

Type 6 is shown by the plots to begin at about the time of the maximum frequency of Type 5; it is in the ascendant at the top of each test. The glazed trait as a whole, however, being decidedly on the wane at the top of each test, we may reasonably consider Type 6 to be the last phase of the industry.

Summing up the foregoing, we see that Type 1 is the earliest; Type 2 is without much doubt transitional between it and Type 3, which latter in turn may develop directly into Type 5; the exact morphological position of Type 4 is still uncertain, but chronologically it is either between 3 and 5 or contemporaneous with early 5; Type 6 represents the last stage of glazing.

The value of glazed ware as an archaeological index for Pecos is now obvious. It was made from almost the beginning to shortly before the end, and it may be subdivided into easily recognized types which are chronologically sequent. Glazed ware can therefore be used for assigning relative dates to burials, refuse beds, rooms, kivas, etc.; and also as a criterion for assigning to their proper chronological positions other wares, types of artifacts and the like, which are found in conjunction with one or another of its phases.[1]

Having made out the succession of the three main ware classes, black-on-white, glaze, and late, and having also arranged the principal types of glazed ware in their proper order, we have built up the framework, so to speak, of the ceramic history of Pecos. The greater part of our work, however, still remains to be done; for it is more important to know how and why the observed changes took place than to have merely learned that they did so. The method to be pursued in further research is clear: we must make an intensive study of the wares themselves. For this we must have not only large amounts of material, but also as nearly as possible unmixed material, from each archaeological horizon. The purest representation of any given period is, of course, to be derived from isolated sites inhabited only during that period. For the earlier stages such sites can easily be found, as in early times the people lived in small scattered communities, which often shifted their locations.[2] The one-period site of later times is, however, much rarer, the Pueblos then having been gathered together in large and more or less permanently established towns whose rubbish heaps contain the inevitably somewhat mixed debris of several periods. Isolated heaps must therefore usually take the place of isolated sites; and, though they are difficult to find, careful search at any large ruin will be rewarded by the discovery of pure deposits.[3]

[1] Black-on-white, biscuit, modern, and even black, are all undoubtedly subdivisible and will eventually also be of use for the above purposes.

[2] *Cf.* "*N*," p. 179.

[3] Most often to be found among the houses, in places where rooms have fallen temporarily into disuse, have been filled with dumped rubbish, and have then been sealed up by the construction of new floors.

For certain transitions, and for periods from which no isolated heaps can be found, we must, however, turn to the large middens, and make stratigraphical tests. To pick out only the best for the illustration of the desired horizons, we must be able to analyze these tests understandingly. To make this point clear, let us consider the ones we have before us.

Test X, according to its glaze-type chart, contains an orderly deposition of Types 1, 2, and 3. Types 4, 5, and 6, on the other hand, are very weakly represented and their curves are far from normal. We conclude, therefore, that at the end of the Type 3 period the heap was practically abandoned as a dump, and that its top cuts accumulated so slowly as to be nearly valueless for close study.[1] We should, then, concentrate on the six bottom cuts for information as to Types 1, 2, and 3. Had we been guided in this case by the total-glaze curve alone, we should have fallen into error, as we would doubtless have believed (because the total-glaze curve falls in this test most nearly to zero) that the upper cuts would give us excellent material for the concluding phases of the ware.

Tests XI and XII are quite different from X, but are similar to each other. The plotted analyses of their glaze types show that their lower parts were built up much more slowly than was the case with X (two cuts from the bottom to the apex of Type 3, as against six cuts in test X); whereas they have five cuts containing the strong curves for Types 4, 5, and 6 (as against two cuts in X with very weak curves). Interpreting this, we see that tests XI and XII came slowly at first, but finished rapidly; they are therefore best for the study of the later glazes. Test XIII is in some ways the most useful of all, though this fact would not be suspected by examining its ware-group chart alone, for there the curve for total-glaze is much like the glaze curves of the other tests. Turning to the glaze-type chart, however, it is seen that following Type 1 there is no representation whatever of Types 2, 3, and 4, Types 5 and 6 coming in abruptly at a relatively high per cent. This shows

[1] It may be considered axiomatic that the faster a heap accumulates the purer and less disturbed will be its contents.

that there was no deposition whatever of refuse, from some time during the life of Type 1, until the comparatively late date when glaze as a trait was on the decline, and the late wares had already come into vogue (see curves for modern, polished black, and plain red).[1] We have, then, in test XIII a long section of pure Type 1 debris, making clear the rise and development of that type, and providing helpful knowledge as to its relation to the precedent black-on-white and the contemporaneous phases of biscuit. This test proves, for example, a thing which we had not hitherto suspected, *viz.*: that biscuit *B* came into limited use before the origin of Type 2 glaze.

We cannot, within the limits of this paper, and with the scanty material now at hand, go further into detail; but enough has been said to show the value of test analysis and the importance of plotting the results graphically.

Recapitulation

The earliest ware found on the Pecos mesa is the black-on-white. With it occurs blackware of the strong blind-corrugated variety and a small percentage of true corrugated, but never as far as our investigations have shown, is there any indented corrugated. Further, the dull-black-on-red and the black-on-white with basket-marked exterior, both classes noted by Mr. Nelson from Tano ruins and by the author from the lesser sites on the Pajarito and near Santa Fe,[2] are not represented at Pecos. They are to be found, however, at "Bandelier Bend," a ruin a little below Pecos on the opposite side of the arroyo, and at several other places near Glorieta. As the corrugated technic gradually fades out at Pecos from the early strata upwards, the most strongly marked specimens there occurring at the bottom, it is reasonable to suppose that the ruins which produce the best grades of the ware are earlier still. Hence we conclude that the Pecos mesa was not settled until toward the close of the black-on-white period. Whether or not glazing

[1] As an illustration of the purely archaeological value of test analysis it may be said that the lacuna in this particular rubbish heap throws very important light on the method of growth of the north end of the Pueblo.

[2] "*N*," p. 171; "*K*," p. 414.

had been discovered or introduced at that time is still an open question. The great preponderance of black-on-white pottery in the early strata prove that glazing could not have attained any considerable vogue, yet the lowest cuts in all our tests show traces of glazed pottery. On the other hand, isolated rubbish heaps of the north terrace, and a black-on-white cemetery on the west slope, contain no glazed sherds at all. We believe, therefore, that for a short time at least, the black-on-white industry was the only one practised at Pecos.

When glazing did come in it very quickly drove out the older style. This is clearly shown by the charts; they indicate, however, that glazing and black-on-white existed for a short time side by side. This is of course only natural, for we have no reason to think that there was an abandonment of the pueblo during the transition, or that any wholesale influx of outside clans brought the change about abruptly.[1]

Glazing was probably not of simultaneous origin in the whole country. It was undoubtedly discovered accidentally at some one site by the mixing of a flux in the pigment, and spread outward from there, the novelty and beauty of the paint aiding rapid dissemination. While it is of course futile to try to locate the point of origin with any exactitude, we believe that it lay to the west, probably in the Little Colorado; our reasons for this belief are as follows: The earliest form of glazing in the Rio Grande was the black-on-red. This has been pointed out by Mr. Nelson, and is confirmed by the Pecos finds. Now black-on-red is exceedingly rare in dull-paint pottery of the Rio Grande black-on-white period; and, if our Pecos observations are correct, it had entirely disappeared during the later phases of that period. Hence, it seems strange that a new paint, if discovered locally, should first have been applied over a slip of a color that was seldom if ever used locally at the time. The southern and western parts of the pueblo district seem always to have been the greatest centres for the manufacture of red wares,

[1] Clan-migration should only be called upon as a last resort to explain pottery changes, for while we know that such movements were common and a very potent factor in the makeup of all the later pueblos, we have no evidence that they were capable of producing radical overturns in resident cultures.

therefore the sudden appearance of a redware with a new form of paint in the east suggests western influence.

The above conclusion is entirely theoretical; to strengthen it, however, we have a few bits of archaeological evidence. In the Little Colorado drainage there occurs a redware with glaze decoration, whose bowls bear characteristic exterior designs in chalky white paint. Although Fewkes and Hough have figured a number of pieces, the chronological position of the style has never been fixed; the vessels, however, bear internal evidence of belonging to a fairly well-developed ceramic industry. We know of four fragments of such vessels from the Rio Grande: one from the Rowe ruin, an almost straight black-on-white site; two from Agua Fria "schoolhouse," which from its preponderance of black-on-red glazed ware we are justified in assigning to an early period; one from cut 7, Test X, a very early stratum, as it contains 25 per cent. of black-on-white ware. These were presumably all trade pieces and so serve to indicate with some degree of probability that glaze decoration was present and perhaps even fairly well advanced in the Little Colorado while it was still a very young art in the Rio Grande.

The locating of the origin of glaze to the west rests, of course, on possibly faulty theoretical grounds, and at best very meagre archaeological evidence. It is, however, a convenient working hypothesis which further research in the western districts and a close study of the designs will either prove or disprove. Whether or not glaze was first discovered in the west, it reached its maximum development in the east; and that development was, as far as we can see, a gradual and purely local one.

Returning to the Type 1 glazed wares, we may repeat that the black-on-red phase was at first the preponderating one. The black-on-light, however, slowly gains in importance and, as we approach Type 2, the red falls away. Type 2 is marked by a degeneration of the glaze, which becomes thicker and less adaptable to the production of fine-line designs; by a parallel deterioration of the clear yellow color of the slip; by a thickening inwards of the bowl rim (which, however, still remains square on top); and by the appearance on bowl exteriors, first of slashed marks and

crosses, then of red-filled decorations. The transitional nature of Type 2, both technologically and stratigraphically, was pointed out in the preceding section. Although the ware is less uniformly specialized than those of our other groups, it appears at Pecos in large quantities, indicating that its manufacture was carried on during a considerable period of time; further, the ware is characteristic of at least one isolated site which contains nothing else.[1] For these reasons we have given it typical rank.

The other wares associated with Types 1 and 2 glaze are the black and the biscuit. Of the former little need be said; it shows a steady elimination of the visible signs of the structural coil and a progressive modification of the rim. Biscuitware presents several interesting features. Biscuit *A*, the early variety, is unslipped on bowl exteriors and bears decorations which seldom if ever conform to the highly specialized design-system of biscuit *B*; but which seem, on the basis of the sherds, to be allied rather closely to those of the black-on-white. That the ware is a development from the black-on-white would naturally suggest itself, and indeed seemingly transitional pieces are not rare. The change from biscuit *A* to biscuit *B* consists in the raising and straightening of the bowl-rim, in the specialization of the design, and in its extension with the slip to bowl exteriors. In general, biscuit *A* is associable with Type 1 glaze, *B* with 2, 3, 4, and very early 5.[2]

Returning to the development of the glazed wares, we see that in Type 3 the slip on visible surfaces is almost invariably light, but is of poor and uncertain quality; the glaze lines also are seldom sharp-edged and clear black, but are heavy, "runny," and of various shades of rusty black, brown, and greeny-brown. Because of the intractability of the paint the interior designs of bowls and the body designs of ollas are considerably simplified; the exteriors of bowls and the necks of ollas, on the other hand, bear handsome decorations (usually oblique variants of the double-ended key figure) in red with glaze outlines. Type 5 seems to be merely a further growth along the same lines, *viz.*: glaze becomes even heavier;

[1] The "Frijolito" ruin, see "*K*," p. 454.

[2] Test XIII shows biscuit *B* to have originated a little earlier than Type 2 glaze.

interior decoration of bowls is simplified to a mere pair of red filled lines encircling the piece just below the lip; exterior design grows more varied; and finally the rim reaches a maximum of thickness.

Found in the same strata that produce the latest forms of Type 3 and the earlier forms of Type 5, is a certain percentage of pieces scarcely, if at all, different from the pottery which in a former paper we have called "Pajaritan,"[1] and have described as Type 4 in the present one. It seems to come in abruptly, runs but a short course, and abruptly disappears, apparently with no preceding or succeeding type. Its high rather thin rims, red color, good glaze, and specialized decoration, do not fit at all well in the Pecos series. It was commonly buried with the dead, as were Types 1, 2, and 3; while Type 5 vessels were almost never so deposited. One of three conclusions might be arrived at:[2] (1) that it was really transitional between 3 and 5, but that we have not sufficient evidence or sufficient acumen to enable us to recognize it as such; (2) that it was a short-lived secondary style, arising from Type 3 and running a course parallel to early Type 5; (3) that it was made by a group of people from the Pajarito who settled at Pecos, as the Hano did at Hopi, were assigned a section of the site, made their own style of pottery, and continued their own mortuary customs; finally losing their cultural identity or moving away. Careful work in the rubbish will, without much doubt, eventually settle this interesting problem.

The wares found in conjunction with Type 5 glaze are the featureless black and, towards the end, the striated black; with its earlier phases, or roughly synchronous with the Pajaritan admixture just noted, there is a considerable amount of biscuit *B*, which seems to average yellower than the earlier biscuit *B* and to have higher rims. This ware rapidly decreases in importance and, shortly after the middle of the Type 5 glaze period, it entirely disappears.

Type 5 continued well into historic times. This is proved by the finding in Type 5 rubbish of the bones of domestic animals

[1] "*K*," p. 417.

[2] Specimens are too common to be considered trade pieces.

and objects of European manufacture. At just what period in the life of Type 5 the Conquest occurred cannot yet be stated, as the bones and artifacts from the tests have not yet been studied, nor can we as yet put a date to the appearance of Type 6, the last stage of native glazed ware.

Type 6 is distinguishable from the preceding one by the diversity and eccentricity of the vessel shapes, and by the extremely "runny" nature of the glaze. We quote Mr. Nelson's excellent description:

> Generally the iridescent glaze substance is of such striking and excellent quality as to incline one to believe that it was compounded after a Spanish formula. The fact that the artist could not control it at all seems suggestive of the same idea. The designs attempted, though of the very simplest geometric nature, were almost invariably spoiled by the running of the glaze. The color and general appearance is a very characteristic dark brown when thickly applied and of a greenish hue when the coating is thin.

The fact that the ware is all of historic date lends much weight to Mr. Nelson's supposition that the glaze was compounded after Spanish formulae. If such was the case, the style must have been a very short-lived one indeed, as Spanish influence strong enough to have seriously affected the manufacture of pottery can hardly be believed to have exerted itself prior to the actual settlement of the country at the beginning of the seventeenth century, and Mr. Nelson has shown that the making of glazed pottery ceased at about the time of the revolt of 1680.[1]

Next came in the dull-paint vessels described above as "modern-painted ware." Their whole appearance, color, pigments, and decorative elements are radically different from those of glazed pottery. The only resemblance is in the high, outcurved rim that is common both to modern and to glaze Type 6. As to the rapidity of the rise of modern ware we cannot yet supply much data, as our tests all tend to become unreliable toward the top. That it eventually completely superseded glazed ware is proved by the total absence of the latter from certain isolated heaps of late rubbish found at Pecos, and by Mr. Nelson's observations at Cienega no. 1 and Cieneguilla.[2] The origin of the style is as yet a mystery. On its

[1] "*N*," p. 175. The Pecos ruin can hardly be expected to add corroboration to this, as the site was not deserted at any time during the rebellion.

[2] "*N*," p. 176 and table p. 179.

first recorded appearance it bears designs that by no stretch of the imagination may be believed to be the direct descendants of any of the glazed designs. Technically it is most nearly allied to biscuit-ware, and some of the decorative elements may possibly be derived from biscuit prototypes. If it is a late outgrowth of biscuit, that growth can hardly have taken place at Pecos, for our test material shows that the making of biscuit had probably ceased completely some time before the modern came into vogue. Mr. Nelson throws out the hint that the type may have been derived from the Keresan region to the north, and this is a particularly useful suggestion in view of the fact that biscuitware was primarily a northern product, reached its maximum development in the upper reaches of the Rio Grande and the Chama, and seems to have continued in use in that district rather longer than it did in the central Rio Grande. It is possible, therefore, that we may yet find that modern painted ware was a normal growth from an ancient prototype, probably biscuit, in some region to the north, and that the unsettled conditions of the rebellion, with their constant movements of people, may have brought about its introduction to the central Rio Grande as an already perfected style. If this is so, we will not be forced to postulate a conscious revolt against what Mr. Nelson calls "the degenerative tendency of the seventeenth century"; or to believe, as we have sometimes done, that the modern ware was the result of an archaistic revival carried out by the Pueblos at the time of the rebellion in a desire to do away with everything savoring of Spanish influence, and to return to the dull-painted wares that observation of the sherds about their villages must have shown them were made by their prehistoric ancestors

That there were changes in the modern ware after its introduction and continuing until the time of the abandonment of Pecos in 1838, can be seen by examining the pottery fragments in and around the houses on the mesa-top. Similar changes evidently also took place in the polished black and the plain red. Late debris, however, in large enough quantities for satisfactory tests, has not yet been found lying conformably upon earlier strata. Such deposits surely exist at Pecos, but until they have been

located and examined it is idle to attempt to carry the study of type-sequence beyond the end of glaze-making times.

TENTATIVE DIVISION OF THE CULTURE-HISTORY OF THE EAST-CENTRAL RIO GRANDE INTO CHRONOLOGICAL PERIODS (the leading ware of each period is italicized).

I. Formative period.	Wares?	Nelson's pre-pueblo, not yet found in Pecos valley.
II. Archaic period....	Indented corrugated, *black-on-white*, black-on-red.	Nelson's I. Not found at Pecos, but present at other nearby sites.
III. Late archaic.....	Corrugated (little), strong blind-corrugated, *black-on-white*.	Nelson's I.
IV. Period of introduction of glaze.[1]..	Strong and medium blind-corrugated, *glaze 1*, biscuit *A*.	Nelson's II. Author's Agua Fria "schoolhouse."
V. Period of concentration.[2].....	Faint blind-corrugated, *glazes 2 and 3*, biscuit *B*.	Nelson's III. Author's "Frijolito."
VI. Late prehistoric. ?–1600.....	Featureless black, glaze 4, *glaze 5*, biscuit *B*.	Nelson's III. Author's "Pajaritan."
VII. Early historic (1540) 1600–1680.....	Striated black, late glaze 5, *glaze 6*, modern begins (?).	Nelson's IV.
VIII. Late period 1680–1840.....	Striated black (?), plain red, polished black, *modern*.	Nelson's V.
IX. Present period 1840–1917.....	Polished wares (Santa Clara), *painted wares* (San Ildefonso, Santo Domingo, Tesuque, Cochiti).	

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[1] And possibly also of the kiva.

[2] Concentration had been going on from the earliest times, but apparently became particularly strong at about this time in the Pecos valley and on the Pajarito. In the Tano country it took place somewhat later (see Nelson's chart, p. 179), perhaps because the Galisteo basin was less exposed to raids from the north and east.

ORIGIN OF THE WORD SHAMAN

By BERTHOLD LAUFER

IN *Chinese Clay Figures* (p. 198) the writer had occasion to discuss briefly the history of our word shaman, and to refer at the close of his naturally succinct note to the new theory of J. Németh, according to which the term should be an ancient constituent of the Turkish-Tungusian languages. The latter reference was an addition inserted in the proofs at the moment when Mr. Németh's article reached me (summer 1914), but since then it has been possible to scrutinize his theory at close range, with the result that it can be confirmed and even be supported with new data and arguments. As ethnologists may be expected to take an interest in the history of a term which has become part and parcel of anthropological nomenclature, and the origin of which has given rise to numerous discussions and speculations, a brief examination of the case might not be unwelcome to the readers of the *Anthropologist*.

There could never be a serious doubt of the real source from which the word shaman is derived. We received it from the Russians: it was the Russian (chiefly Cossack) explorers and conquerors of eastern Siberia in the second half of the seventeenth century, who heard and recorded the term among Tungusian tribes. It was first brought to Europe by the Hollander, E. Ysbrants Ides, and by Adam Brand, who from 1692 to 1695 accompanied a Russian embassy sent by Peter the Great to China. Some examples from the former's *Driejaarige Reize naar China*, first published at Amsterdam, 1698 (again 1704 and 1710), may be cited:

> Eenige dagen reizens van hier, is de groote steenachtige waterval, Schammanskoi, of Toverval geheten, om dat aldaar een beroemde Schamam, of Tungusche duiveldienaar woont (p. 34).
>
> Eenige mylen van hier opwaarts woonen veele Tunguzers, waar onder ook hun beroemde Schaman, of Duivelskonstenaar (p. 35; follows a lengthy description of the shaman and his costume).
>
> Zy [the Tungus] weeten van geen andere Priesteren, dan hunne Schamannen, of Duivelbanners (p. 39).

It is thus shown that our "shaman" had its origin in the Tungusian languages, in all of which the word indeed is known as *šaman* or *saman*, and with several other variations to be noted hereafter. It would be reasonable to regard the term as of native origin, for it is not likely that the name of a religious institution and function so characteristic of all the tribes of northern Asia should be borrowed from an outside quarter. Nevertheless it was possible that at a time when little was known about Siberian shamanism, and when rigid philological principles were not yet established, the theory could be advanced that Tungusian *šaman* or *saman* were to be derived from Chinese *ša-men*, itself a transcription of Pāli *samaṇa* corresponding to Sanskrit *çramaṇa*, a technical term for the designation of a Buddhist monk or ascetic. The germ of this idea was well prepared as early as the eighteenth century. The Augustinian Pater A. Georgi[1] states correctly that the Samanaei mentioned by Clemens Alexandrinus[2] are the adherents of Buddha, and in this connection refers to La Croze, who already hazarded the combination of the Samanaei with the shamans of the Tungusians ("le nom de schaman est un nom de religion d'un peuple tartare, voisin des frontières de la Chine"). Sanskrit not yet being well known at the time of La Croze and Georgi, the origin of the Greek term Samanaioi escaped them; this is what the philologists of the nineteenth century supplied.[3]

Which of them was the first to assert the connection of the Siberian *šaman* with the Indian term I am unable to ferret out; this point is immaterial, as the question is merely that of the history of an error. D. Banzarov[4] was inclined, without giving a specific reference, to fix the responsibility on F. Schlegel, according to whom

[1] *Alphabetum Tibetanum* (Rome, 1762), p. 222.

[2] In fact Clemens avails himself of the form Sarmanai.

[3] Maturin Veyssiere La Croze was born at Nantes in 1661, joined the clergy, suffered persecution on account of his liberal views, fled to Germany, where he turned a Protestant in 1696, and was made a member of the Academy of Sciences of Berlin, where he died in 1739. His work *Histoire du Christianisme des Indes*, to which Georgi refers, unfortunately is not accessible to me. La Croze was a scholar of wide knowledge and linguistic ability: he was the first who had a presentiment of the relationship of Indian with Persian.

[4] *Černaya v'ăra*, p. 34 (see p. 366).

the *çramaṇa* among the crude tribes of Central Asia transformed themselves from hermits into shamans. I believe that Banzarov had in mind Friedrich von Schlegel (1772–1829), romantic poet and Sanskrit scholar, who published in Heidelberg, 1808, an essay under the title *Über die Sprache und Weisheit der Indier, ein Beitrag zur Begründung der Alterthumskunde*.[1] This is the only one of his works in which such a statement might be supposed to occur; but on a cursory perusal of this book I am unable to trace it. The idea itself being romantic, there would be no reason to wonder that it took its root in the Romantic School. At all events it was alive in the first part of the nineteenth century. The French sinologue J.-P. Abel-Rémusat (1788–1832) became a champion of this theory.[2] An ingenious scholar of the type of J. Klaproth[3] was capable of saying, "Les peuples tartares ont sans doute reçu le mot *chaman* de l'Inde avec le bouddhisme, car il est indien d'origine, et signifie un homme qui a vaincu toutes ses passions." In 1857 Max Müller[4] of Oxford wrote,

> Shamanism found its way from India to Siberia *viâ* Tibet, China, and Mongolia. Rules on the formation of magic figures, on the treatment of diseases by charms, on the worship of evil spirits, on the acquisition of the supernatural powers, on charms, incantations, and other branches of Shaman witchcraft, are found in the Tanjur, or the second part of the Tibetan canon, and in some of the late Tantras of the Nepalese collection.

Needless to add that the magical writings of the Tanjur are unknown to the shamans of Siberia and have nothing to do with Siberian shamanism. Still less is it intelligible how Müller could say that "the only trace of the influence of Buddhism among the Chudic races, the Finn, Lapp, etc., is found in the name of their priests and sorcerers, the shamans." The word shaman is foreign to any Finno-Ugrian language; it found its way into Finnish (*shamani*) and Hungarian as a scientific term only, in the same

[1] An English translation was edited in London, 1889, in *F. v. Schlegel's Aesthetic and Miscellaneous Works*.

[2] *Recherches sur les langues tartares* (Paris, 1820), p. 133, and *Observations sur la doctrine samanéenne et la triade suprême* (Paris, 1831).

[3] *Mémoires relatifs à l'Asie* (Paris, 1828), vol. III, p. 67.

[4] *Chips from a German Workshop*, vol. I, pp. 233–234.

manner as into the Indo-European languages. A. H. Sayce[1] has the following:

> In shamanism, so called from the Shaman or Siberian sorcerer, who is himself but a transformed *çramaṇa*, or Buddhist missionary priest, we rise to a higher conception of religion.

These superstitions were tenaciously upheld under the influence of the romantic movement of pan-Indianism, which held the minds of scholars enthralled in the first part of the nineteenth century, and the germs of which are not yet entirely exterminated.

True it is that the Sanskrit word *çramaṇa* with the Prākrit form *samaṇa* has become widely known outside of the pale of India. It reached the ears of the Greek writers on India, who described the Buddhist monastics under such names as Sarmanes, Sarmanai, or Samanaioi. The first of these names was recorded by Megasthenes,[2] though the single text of Strabo from which all the existing codices have been copied offers the erroneous transcription Garmanes.[3] The word has also passed into early Arabic records for the designation of the Indian and Bactrian Buddhists (for instance, in Masūdi, Gurdezi, and Abu'l Faradj) in the form *samaniyya*, while in later literature the form *šaman* was adopted.[4] In Firdausī's Shāhnāme, completed in A.D. 1010, we find *šaman* in the sense of a worshipper of idols, and this Persian word likewise is traced to Sanskrit *çramaṇa* and its congeners.[5] Again, it bears no relation to "our" or the

[1] *Introduction to the Science of Language*, vol. II, p. 293.

[2] Strabo XV, 1, 59.

[3] The texts in question are easily accessible in J. W. McCrindle, *Ancient India as described in Classical Literature*, pp. 65, 168, 170. See further C. Lassen, "De nominibus quibus a veteribus appellantur Indorum philosophi," in *Rhein. Museum für Philol.*, vol. I, 1833, pp. 178–180, and *Indische Alterthumskunde*, vol. II, p. 700; E. Burnouf, *Introduction à l'histoire du Buddhisme indien*, p. 245; T. Watters, *Essays on the Chinese Language*, p. 403; S. Lévi, *Journal asiatique* (1911), I, p. 445.

[4] Geza Kuun, *Keleti Szemle*, vol. IV, 1903, p. 133; G. Ferrand, *Textes géographiques arabes relatifs à l'Extrême-Orient*, vol. I, p. 131. The Mohammedan writers were perfectly conscious of the fact that the term hailed from India and denoted the Buddhists of India and China. See particularly D. Chwolsohn, *Die Ssabier und der Ssabismus* (St. Petersburg, 1856), vol. I, pp. 165, 214, 217, etc. As far as I know, the shamanistic religion of Siberian tribes is not mentioned by any early Arabic or Persian author.

[5] P. Horn in *Grundriss der iranischen Philologie*, vol. I, pt. 2, p. 7; K. Vullers (*Lexicon persico-latinum*, vol. I, p. 466) renders the term by *idololatra* and *idolum*.

Siberian shaman, but evidently refers to Indian Buddhists, for these šamans say, "Our religion is one of peace and quiet, and fighting and slaying is prohibited, as well as all kinds of shedding of blood."[1] Finally the Indian word has been traced in the newly discovered Indo-European language styled Tokharian in the form *ṣamāne;* and this induced A. Meillet to formulate the opinion that the Tokharian form (not a Prākrit form of Sanskrit *çramaṇa*) would account for the Tungus word *šaman*.[2] With all respect due to the scholarship of this eminent philologist it must be admitted, however, that there is a far cry from Tokharian to Tungusian, that there is no evidence for any contact of these two groups, and that the Tungus notion of a shaman is radically different from the conception of a Buddhist monk. If two words in geographically remote languages are physically alike or similar, but fundamentally diverse in meaning, it is safe to assume that the face resemblance is purely accidental.

Indeed this theory was brilliantly antagonized as early as 1842 by W. Schott; and H. Yule,[3] accepting his verdict, reached the conclusion, "Whether the Tungus word is in any way connected with this or adopted from it [that is, the Sanskrit-Pāli term], is a doubtful question." Schott has devoted no less than three studies to this problem,[4] in which he endeavored to dissociate *šaman* from *samaṇa*. His arguments are briefly as follows: the Chinese strictly discriminate between Manchu *saman* (shaman) and Sanskrit *çramaṇa*, and never confound a shaman and a Buddhist monk, the two having totally diverse functions; a direct or indirect connection of Tungusian tribes with India is unproved; Buddhist missionaries never advanced into regions inhabited by Tungus;[5]

[1] Compare the quotation in H. Yule, *Hobson-Jobson*, p. 820.

[2] A. Meillet, "Le Tokharien," *Indogermanisches Jahrbuch* (Strassburg, 1914), vol. I, p. 19.

[3] *Hobson-Jobson*, p. 820.

[4] "Über den Doppelsinn des Wortes Schamane und über den tungusischen Schamanen-Cultus am Hofe der Mandju-Kaiser," *Abhandlungen Berliner Akademie*, 1842, pp. 1–8; "Wohin gehört das Wort Schamane?" in his Altaische Studien, no. 3, *ibid.*, 1867, pp. 138–141; and "Das Wort Schamane," in *Erman's Archiv für wissenschaftliche Kunde von Russland*, vol. XXIII, 1865, pp. 207–210.

[5] As emphasized in my article "Burkhan," *Journal American Oriental Society* (1917), pp. 390–395, there is no trace of Buddhism in the religion of the Tungusian peoples on the Amur.

it would be strange if they should have borrowed the only word in their language for the designation of their national priests; the forms of shamanism are uniform in a vast territory extending from Lappland to the Bering Sea and far beyond into America; Vasilyev's hypothesis that shamanism should be a caricature of ancient Buddhist sorcery and jugglery and have been brought into existence by such influences is a mere phantom; the outward resemblance of the words *saman* and *samaṇa* is purely fortuitous. Schott's etymological explanations of the word from Samoyed are not convincing, but his remark that Tungusian *sam* possibly is anciently related to Turkish *kam* testifies to great foresight and ingenuity; and this thesis, as will be seen, can now really be proved. Also Ch. de Harlez[1] joined the ranks of the adversaries of the Indian hypothesis, reiterating in the main Schott's arguments. In Russia, it had been severely criticized as far back as 1846 by Dordji Banzarov, a Buryat and an excellent Mongol scholar.[2] Nevertheless in Russian dictionaries also the nightmare of the *çramaṇa* is perpetuated.[3] As it happens, twilight reveries appeal more to the multitude than plain reason and continue to live even if abandoned by the thinking minority; it is not always the best and fittest that survives. The ghost of the Indian *çramaṇa* still haunts the poor shaman in our standard dictionaries and cyclopaedias (see Webster, Nouveau Larousse, and Littré, *Dictionnaire de la langue française;* Schaff-Herzog *Religious Encyclopaedia*, vol. x, p. 385; Funk and Wagnalls' Dictionary offers even a "Persian-Hindustani shaman, pagan"!).[4] It is a relief, therefore, to read in the new Oxford Dictionary,

> The Persian *shemen*, idol, idol temple, sometimes cited as the source, is unconnected. Evidence seems to be wanting for the plausible suggestion that the Mongolian word is an adoption of Chinese *sha men*, an ordained member of a Buddhist fraternity.

Eliminating "plausible" and substituting "Tungusian" for

[1] *La Religion nationale des Tartares orientaux* (Bruxelles, 1887), p. 28.

[2] His works were collected and edited by Potanin in 1891 under the title *Černaya v'ära ili šamanstvo u Mongolov i drugiya stat'i* ("The Black Faith or Shamanism among the Mongols and Other Articles"). Regarding the word shaman see p. 34.

[3] For instance, in Gor'aev, *Sravnitelny etimologičeski slovar' russkago yazyka*, p. 418.

[4] Even in the 7th edition of O. Peschel's *Völkerkunde* (p. 274) the *çramaṇa* still holds sway.

"Mongolian," this statement marks a progress in lexicographical literature. So there is hope that our great-grandchildren will perhaps be treated to a correct definition in the dictionaries of the following century.

The Hungarian scholar J. Németh[1] has attacked the problem in the right spirit, and ably solved it by means of a sane philological method. A schooled phonetician, he has successfully worked for several years on the phonetic history of the Turkish and Mongol languages and established very interesting correspondences of phonetic phenomena in these two linguistic families. In the study cited he advances the law that primeval Turkish initial *k'* (palatalized *k*) has developed into Tartar *k*, Čuvaš *j*, Yakut *x* (spirant surd as in German *ach*), Mongol *ts*, *č*, and Manchu-Tungusian *s*, *ś*, or *š*. He gives seven examples to this effect, the first of which is:

Turkish (Uïgur and in the dialects of southern Siberia) *kam* or anciently *qam* (shaman), in some dialects *küär*, *kum* (with such derivations as *kamdi*, to prophesy; *kamla*, to heal; *kamna*, to shamanize; etc.). Čuvaš *jum*, *jumeś* (shaman); *im-jum* (curative charms). Manchu *saman*, Golde *šama*, *ś'ama*, Olča *šam*, Oročon *s'ama*, Tungus on the middle Amur *samaň*, Tungus of Udsk *saman* and *šaman*, Kondogir and Wilui Tungus *xaman*.

Németh concludes that the origin of the much disputed word "shaman" is thus definitely solved, and that it is an ancient property of the Turkish-Mongol languages. The phonetic alternation determined by him is perfectly correct; and Manchu-Tungusian *sam*, *s'am*, or *šam*[2] is indeed the exact phonetic equivalent of Turkish *kam*.[3] Apart from the Tungusian series noted by Németh

[1] "Über den Ursprung des Wortes Šaman und einige Bemerkungen zur türkisch-mongolischen Lautgeschichte," *Keleti Szemle*, vol. XIV, 1913–14, pp. 240–249.

[2] This in fact is the stem of the word, not as formerly believed by some scholars, *sa-*, which was explained from Manchu *sa-* (to know). Németh justly rejects this derivation. By the way, Schott, the father of this etymology, disavowed it himself in his second treatise on the word shaman, quoted above. D. Banzarov (*l. c.*, p. 35) justly recognized *sam* as the stem.

[3] It was hitherto believed that the word for shaman was different in all languages of Siberia, while there is a fairly uniform designation for the female shaman (*udagăn*, *ulygan*, *ubakxan*, etc.). Hence V. F. Troščanski (Evolutsiya černoi v'ăry [šamanstva] u Yakutov = "Evolution of the Black Faith among the Yakut," Kazan, 1902, p. 118) was prompted to the theory that the Siberian tribes originally had only female shamans, and after the time of their separation instituted also male shamans. This reconstruction is contestable on many grounds; for instance, we have male and female shamans

after Grube's *Goldisches Wörterverzeichnis*, several other forms of the word have become known. De la Brunière[1] recorded it in 1846 among the Golde as *tsama* or *tsamo*. I heard only *s'ama* from the Golde, in the same manner as Shimkevič,[2] but in view of the existence of several Golde dialects such a form may be regarded as possible. A. Rudnev[3] noted from a Manchu, who was still conversant with his mother tongue, the following: *sáma*, *sáman*, *sámą* (nasalized *a*), and *sāmăn*.

The Gilyak style the shaman *čam* (*tšam*). This certainly is a loan-word received from Tungusian,[4] but it is not necessary to assume that the change of the initial *š* or *s* into the palatal explosive surd is due to the Gilyak; for this alternation is common within Tungusian, so that a Tungusian *čam* borrowed by the Gilyak may be presupposed for some Tungusian dialect spoken in the neighborhood of the Gilyak. Compare the following examples: Manchu *nesuken* and *nečin* (equal)—Jučen *nušin;* Manchu *ničuxe* (pearl)—Old Jučen *inšuko* or *inšuxa*, modern Jučen *ninčuxe;* or *vice versâ*, Jučen *ančun* (gold)—Manchu *aisin*.

It has been said that the word *šaman* is found only in the eastern part of Siberia and in Manchuria. There is, however, one well-authenticated instance on record to the effect that it was known also in northwestern Siberia. In the earliest account that we have of the Irtysh-Ostyak, written in 1715 by G. Novitski, their priests were styled *šamančik*.[5] In view of the early date of this record we cannot well assume that this is a loan-word received through intercourse with the Russians. Whether the term still exists I

co-existent in the most ancient days of China. The supposition on which the conclusion is based is now no longer valid: the word for the male shaman is in fact identical throughout from the Turkish to the Tungusian and Gilyak tribes.

[1] In H. E. M. James, *The Long White Mountain or a Journey in Manchuria* (London, 1888), pp. 433, 434.

[2] *Materialy dl'a izučeniya šamanstva u Goldov*, p. 8.

[3] *Novyia dannyia po živoi mandžurskoi r'ači i šamanstvu* (*New Data on the Live Manchu Speech and Shamanism*), p. 9.

[4] There is no doubt that the shamanism of the Gilyak is derived from and a weak echo of that of the neighboring Amur tribes. There are but a few shamans among the Gilyak, and shamanizing is rarely practised by them (see for the present L. von Schrenck, *Reisen und Forschungen im Amur-Lande*, vol. III, p. 752).

[5] B. Munkácsi, *Keleti Szemle*, vol. IV, 1903, p. 88.

am unable to say from the sources at my disposal. S. Patkanow[1] gives for the shamans of the Ostyak only the name *tonx-ort*.[2]

The Turkish word *kam* is of ancient date.[3] It is attested (and this is its earliest occurrence) in the Chinese Annals of the T'ang dynasty (618–906), where it is said with respect to the Kirgiz[4] that they designate their sorcerers or shamans (Chinese *wu*) with the word *kam* 呼巫爲甘. The last character now reads *kan*, but was anciently possessed of a final labial nasal, which is still preserved in the dialects of southern China. Chinese *wu* is a very ancient term for the native medicine-men.[5] The word *kam*, further, is found in Uigur[6] and in the *Codex cumanicus* of 1303.[7] It is also used by the Persian historian Rashid-eddīn (1247–1318) in 1302, with reference to the shamans of the Mongols.[8] The existence of the Tungusian word *šaman* can now be traced to the first part of the twelfth century, for Pelliot[9] has found it as a Jučen word in a Chinese gloss, where it is transcribed in the form *šan-man* and explained as a sorceress. The Chinese transliteration would presuppose a Jučen form *šamman*. Indeed we still meet forms with double *m* in the dialect of the Wilui Tungus,—*samman* and *xamman*.[10] The latter with initial spirant represents the missing link between Turkish *kam* (in the language of the Soyot or Urang-khai *xam*) and the Tungusian forms with initial sibilant. The

[1] *Die Irtysch-Ostjaken*, pt. 1, p. 120.

[2] Consulting Castrén's vocabulary in his *Versuch einer ostjakischen Sprachlehre* I find that *tonx* means idol, and *ort*, workman, slave. If accordingly *tonx-ort* should mean servant of the idols, the term might hardly be claimed as a primeval designation of the shaman.

[3] The Russians have derived from it the verb *kamlat'* (to shamanize, to conjure, to spell, to prophesy) and the noun *kamlanie* (the act of shamanizing).

[4] Regarding this name and its ancient Chinese transcriptions see Pelliot and Laufer, *T'oung Pao*, 1916, p. 295.

[5] Regarding their activity see *Chinese Clay Figures*, pp. 198–200.

[6] In the Kudatku Bilik, written in A. D. 1068 (H. Vámbéry, *Uigurische Sprachmonumente*, p. 221).

[7] W. Radloff, *Aus Sibirien*, vol. II, p. 67. Németh overlooked these data, but it is important to note the wide distribution of the word in time and space.

[8] F. von Erdmann, *Temudschin*, p. 553; E. Blochet, *Introduction à l'histoire des Mongols de Rashid Ed-Din*, p. 165.

[9] *Journal asiatique*, 1913, mars-avril, pp. 466–469.

[10] Recorded by R. Maack, *Wil'uski okrug* (*The District of Wilui*), pt. 3, p. 116.

alternation of *k* and *x* is well known;[1] likewise *x*, *h*, and *s*, alternate in Tungusian dialects.[2] For this reason it is justifiable to correlate the forms *kam* and *xam* with Yakut *xamnā*, *xamsā* (to move, to be agitated; *xām*, to step, to stride),[3] and to interpret the significance of the word from the shaman's peculiar behavior, his solemn and pompous pacing, his ecstasy, his convulsions, his frantic leaps and dancing.[4]

Németh gives no form of the word for Mongol, and explains the absence of a corresponding word in this language through a characteristic trait of the Mongols to whom the use of many words was tabooed; owing to such a taboo, the word *šaman* should have been lost. This is an arbitrary, far-fetched, and unnecessary theory. First of all, the word *šaman* does occur in Mongol, at least it is listed as a Mongol word in Kovalevski's and Golstunski's Mongol dictionaries. Whether it is frequent is another question. I never found it in a Mongol text. This is accounted for by the fact that the common term for a shaman in Mongol is *bügä*, *bögä*, Buryat *buge* and *bō*, corresponding to Turkish *bögü*, Djagatai *bügi* (sorcery), *bügiči* (sorcerer), Osmanli *büi* (sorcery).[5] This series seems to bear some kind of relation to Chinese *bu*, *wu* (shaman), *buk*, *puk* (to divine), and Tibetan *ạba* (pronounced *ba*, sorcerer);[6] but how

[1] W. Radloff, *Phonetik der nördlichen Türksprachen*, p. 108.

[2] A. Schiefner, *Mélanges asiatiques*, vol. VIII, 1877, pp. 338–339.

[3] O. Böhthingk, *Jakutisches Wörterbuch*, p. 79.

[4] This explanation was first given by V. F. Troščanski, *Evolutsiya černoi v'ăry (šamanstva) u Yakutov* (*Evolution of the Black Faith among the Yakut*), (Kazan, 1902), p. 117. D. Banzarov (*Černaya v'ăra*, p. 35) held a similar view, but arrived at it through a different process by connecting the word with Manchu *samarambi* and Mongol *samoromoi*, *samagu;* aside from the fact, however, that these words do not have the meaning required, or forced into them by Banzarov, they can by no means be reduced to the stem *sam.—Kam* is not a Yakut word for the shaman, as wrongly stated by A. van Gennep ("De l'emploi du mot 'chamanisme,'" *Revue de l'histoire des religions*, vol. 47, 1903, p. 52); the Yakut term is *oyun*.

[5] According to I. J. Schmidt (*Geschichte der Ost-Mongolen*, St. Petersburg, 1826, p. 416), *kami* should be the Mongol name of the shamans. It would hardly be necessary to point out this blunder, had it not been immortalized in Yule's *Hobson-Jobson* (p. 820). There is no Mongol word *kami* with such a meaning. Schmidt found it in the Mongol chronicle of Sanang Setsen as the proper name of a Tibetan Bon-po, and this name reads correctly Khami or Xami. Schmidt's interpretation, evidently suggested by Turkish *kam*, is baseless and purely imaginary.

[6] Compare my concordance in *T'oung Pao*, 1916, p. 68.

this relation is to be explained is beyond our present knowledge. The type *kam—sam* is possibly preserved in Mongol in a somewhat disguised form. According to the phonetic law established by Németh, the Mongol initial corresponding to Turkish *k* and Tungusian *s* should be *ts*, *č*. Thus we get at Mongol *tsam*, *čam*, that means a dance, and at present refers in particular to the religious pantomimic dance-performances of the Lamas in which, as is well known, many ancient shamanistic notions and rites of pre-Buddhistic times have survived. The same word in the same sense is encountered in Tibetan as *č'am* or *ts'am* (written *ąč'am*), and it would be justifiable to regard the Mongol as a loan-word adopted from Tibetan;[1] but considering the fact that the Tibetan word is isolated (that is, not found in any other Indo-Chinese language), it is likewise permissible to stamp the Tibetan as a Mongol loan. For the rest, I give with all reserve this theory of a possible connection of Mongol *tsam* with Turkish-Tungusian *kam—sam* (religious dancer).

At any rate it is obvious that the word *šaman* has now legitimately secured an absolute and irrevocable decree of divorce from its pseudo-mate *çramaṇa*, *samaṇa*, or *ša-men*, and that this mismated couple cannot live together any longer. Tungusian *saman*, *šaman*, *xaman*, etc., Mongol *šaman*, Turkish *kam* and *xam*, are close and inseparable allies grown and nourished on the soil of northern Asia,—live witnesses for the great antiquity of the shamanistic form of religion.

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[1] In a forthcoming study on *Loan-Words in Tibetan*, where an almost complete list of Mongol loan-words occurring in Tibetan is given, the writer has set forth the difficulties in recognizing the mutual loan-words of the two languages. Mongol and Tibetan have a certain number of words in common, partially due to a prehistoric contact, partially due to a lively intellectual interchange in historical periods; and it is not always possible to decide in each and every case which side is the recipient, and which the borrower.

THE POSITION OF THE BODY IN ABORIGINAL INTERMENTS IN WESTERN MASSACHUSETTS

By HARRIS HAWTHORNE WILDER and RALPH WHEATON WHIPPLE

SOME eleven years ago the senior author published in this periodical an account of excavations in an aboriginal cemetery in North Hadley, Massachusetts, with some details concerning the method of interment of two skeletons found there.[1] At that time the examination was made by simply uncovering the bones, and photographing them as they lay in the damp sand, noting at the time the points of the compass, and the general relations of the body. As the bones were removed separately the same day, the time allowed for study of the position was short, and the observations were made under field conditions, which are never ideal for careful work.

Since then the incompleteness of this record has become more and more apparent, and the idea has gradually developed of finding a typical aboriginal grave, boxing it up in the field, and removing it entire to the laboratory, where, under ideal conditions, the actual excavation could take place. Under such circumstances the bones could be carefully uncovered without the slightest disarrangement, and the whole interment be fixed in place through impregnation with some cementing fluid, like glue or shellac.

Although this plan was conceived in the rough soon after the excavation of 1905, its execution was delayed until the fall of 1915, when opportunity again presented itself for continuing excavation at the same site. For this the two authors, representing respectively the neighboring colleges of Smith and Amherst, associated themselves, and in the work, which has continued since this time and is still being prosecuted, the method of removing entire graves,

[1] *American Anthropologist*, (N. S.), vol. 7 (1905), pp. 295–300, with plate XXIII and a map of the site.

with the objects in place, rather indefinite at first, has become so far developed by the aid of improvements that have constantly suggested themselves, as to form a special archaeological technique, which renders plain and simple certain relationships hitherto extremely difficult to study, and impossible to record permanently. For example, one may study the various mechanical causes of the displacement of the bones, differentiating the natural results of decay, with the dropping and sagging of the parts, and the complications due to the retentive force of the ligaments, from such external forces as frost, the action of earthworms, and the displacement caused by the larger burrowing animals. This being possible, the position of the body at burial, even to certain exact details, may be estimated, and such external conditions as the confinement of the limbs, or the folding up of an already stiffened body by the use of force, be satisfactorily made out.

Aside from all this, which forms almost a distinct line of study, the simple problem of getting out the bones with the least damage is solved most satisfactorily by this method, and many a crumbling fragment of bone, or even a mass of earth which retains the shape of a bone or bone detail, hopelessly lost when working in the field, may be saved when at a laboratory table where cements, glues, and similar substances are always at hand.

It would seem, too, that this method of removing an entire grave would be of advantage in the study of neolithic graves elsewhere, especially where there are associated objects, the exact relationship of which to the body might be important. Graves of neolithic or bronze age peoples appear in many European museums, the bones and artifacts taken from the earth with care, and reassembled in a glass case as accurately as possible by the aid of photographs, drawings, and field notes. Of how much greater value would these be if it could be confidently stated that every piece had remained in its position as found at the time of excavation, without the slightest dislocation.

The present paper deals with the excavation of two aboriginal graves, dealt with in this way, and now placed in the museums of the two colleges in question; putting special emphasis on the position

of the body in local interments, but incidentally illustrating the method of excavating above referred to.

The first of these was found near the Connecticut river, at North Hadley, at the site of the excavations of 1905, but farther out, upon the oval knoll shown in the map of the previous paper[1] there designated as a house-site or village. This was the traditional site of the Nonotuck chief, Quonquont, whose name appeared on several of the deeds for the meadow land, sold to that shrewd English trader from Springfield, John Pynchon. Quonquont was dead by October, 1672, but a deed of that date mentions his widow, Sarah Quonquont; a son, Pocumohouse, and a daughter, Majessit, together with two others, one being a squaw. These seem to have lived at the time at this site, and undoubtedly the village had numerous inhabitants until the defeat of Phillip in 1676 caused the abandonment of the English lands by all Indians who, like these Nonotuck, had taken part in the great confederation.

This combined site, the knoll, the field, and the neck of land connecting the two, has long been known to contain Indian skeletons, and on a number of occasions several at a time have been excavated, in excellent condition. Here the authors dug, during several days in the autumn of 1915, running about four hundred feet of exploratory trenches, and on the afternoon of November 26, a fragment of the outer condyle of a left tibia was thrown up by the shovel.

This led to the location of what proved later to be a complete male skeleton, with arms and legs folded up; beyond all doubt an aboriginal interment. The surface earth was carefully removed to within a few inches of the bones, and these were uncovered at a sufficient number of points to enable us to get the position of the skeleton, after which the whole was covered up again. As the skeleton lay in damp sand, packed down hard, there was in this process no risk of disturbing a single bone, and after the few points of exposure were covered again and the sand packed down upon them, the grave was still in its original condition, with the single exception, of course, of the condyle of the tibia, which had led to the discovery.

[1] *loc. cit.*, p. 296.

A deep trench was then dug entirely about the grave in the form of a quadrilateral, 4 ft. by 2.5 ft., enclosing the entire grave and leaving everywhere a margin of at least six inches beyond the probable position of the bones. A frame of stout planks was then built around the grave, and the whole boarded over for the night.

The next morning a floor was driven across, just beneath the lower edge of the frame, from one side to the other, using for this purpose boards with pointed ends, and digging away the earth from in front of each board as it was pushed in. Before beginning this, two long beams were laid lengthwise along the sides just below the frame, and the floor boards were so placed as to lie just above these, and rest across them. The entire structure, frame and floor, with the enclosed grave, thus came to rest upon the two beams, and when these were lifted by some six men, the whole was readily loaded upon an auto-truck.

In this form the aboriginal grave, weighing about six hundred to seven hundred pounds, was brought to the anthropological laboratory of Smith College, where the actual excavation took place at leisure, and under the most favorable conditions possible. The tools employed for the rougher work were wooden skewers, obtained at the market; also knives and scalpels could be employed when the act to be accomplished was definitely known, and where the bones were exactly located. The use of several sorts of brushes, especially the ordinary type of clothes brush, was of great assistance, and was fairly safe.

This work stretched over the available time of several days, and upon its completion the exposed surface was thoroughly impregnated with shellac, used very thin at first, followed by stronger and stronger solutions, which eventually bound everything in place, earth, bones and all. In order to search for artifacts the earth outside of the boundary of the bones was carefully removed, but was later replaced, mixed with cement, and formed a natural frame about the skeleton, which thus came to lie in a depression, as at first. The specimen has been placed in an oak case under plate glass, and is oriented as nearly as possible in the position relative to the points of the compass that it was when found.

While still lying in its crude box, but thoroughly excavated, the skeleton was photographed from directly above by means of an improvised staging, and the results are here given (fig. 56). To get the proper effect the photograph should be held horizontally with the eye of the observer directly above its center. The skeleton is lying upon its right side, with knees drawn up to the chest, and with the lower legs strongly flexed, and evidently resting on the hams when in the flesh. The arms are also flexed, with elbows touching the thighs; and the hands, placed closely together but not clasped, are close to the face.

In the original orientation the axis of the trunk, or of the entire body as a whole, lay north and south, with the head towards the south, while the face was turned directly towards the east, as in the case of the skeleton figured in my former article.[1] A comparison of the two cases shows a close similarity in the position, even to details.

If this may be taken as the customary position of the body in local aboriginal interments, in the case of males at least, and this seems extremely probable, some explanation should be advanced to account for it. The embryonic theory, that is, that the position of the *fetus in utero* is symbolic of the rebirth, although cited as probable in the previous paper of the senior author, has been rendered untenable through the searching criticism of Andrée.[2] The first mention of the folded body as the embryo was by Peter Kolbens in 1745 who used the comparison simply as a simile, without the slightest intention of suggesting any symbolism on the part of the Hottentots, of whom he spoke. This mention, wrongly interpreted as an explanation rather than as a figure of speech, started the theory, which seems to have nothing in folklore or primitive philosophy to confirm it, save the general belief in the earth as the All-Mother.

A careful study of this absolutely reliable specimen interment strongly suggests that the body when buried was securely tied up

[1] *Ibid.*, pl. XXIII, fig. 2.

[2] Andrée, Richard; "Ethnologische Betrachtungen über Hockerbestattung," *Archiv für Anthropologie* (N. F.), Bd. VI (1907), pp. 282–307.

FIG. 56.—Aboriginal skeleton excavated at North Hadley, Mass., in November, 1915. The bones are exactly as found, as they have never been disturbed. Now in the Zoological Museum of Smith College. The photograph was taken from directly above the specimen, and should be held horizontally to put it into the proper position.

FIG. 57.—Aboriginal skeleton (Pocumtuck tribe), excavated at Greenfield, Mass., November, 1916. Condition and manner of photographing the same as in figure 56. Now in the Gilbert Indian Museum of Amherst College.

by what must have been heavy straps, probably of leather, as otherwise it would be hard to see how a dead body could be forced into such a position. Furthermore, while the most of the bones are either in their exact position or have been simply dropped through the natural decay of the body, the cervical vertebrae, and with them the clavicles and sternum, have been much dislocated, a condition perfectly explicable by assuming the presence of a tight strap, passed under the knees, and over the nape, forcing the head and neck down into a definite stoop. The gradual release of the joints of the body through decay, allowing the free parts to follow more the tension of the ligaments, while still confining the neck vertebrae, would produce exactly the result found in this case, and indicated with some definiteness in the photograph.

The fact that marked disarrangement of bones is shown mainly in this region is made especially significant from the study of other parts of the skeleton, where are shown only the definite results of the effect of decay and the influence of gravitation, and these clearly and vividly. Along the thorax, for example, when decay softened the ligaments which held the ribs to the vertebral column, this latter part rolled somewhat outward, parting from the heads of the ribs, which rested firmly in the earth, and had no tendency to move. This brought the vertebrae of this region up until the ventral aspect of the centers looks almost directly upwards, while the ribs of the upper side, the left ones, have dropped downwards in a bunch. The dropping down of the left knee, and the slight rolling over of the left foot, every phalanx of which has been preserved, are the natural results of gradual decay in place, while in contrast to these the strong dislocation of the vertebrae and other bones in the region presumably bound down by the thong is very striking.

There are two noticeable breaks, aside from that of the left tibia, previously spoken of, which need explanation, and these are (1) the fracture of the left femur near the proximal end, and (2) that of the left humerus, near the elbow. These are evidently comparatively late injuries, and are easily accounted for by the facts that this skeleton lay directly in the driveway which formed

the only convenient means of reaching the field with a wagon, and over which several generations of Hadley farmers have passed up and down with their loaded carts, and that the burial, as is customary, was superficial, the highest parts being not more than twelve to fifteen inches below the surface. In fact, on the day of the find we drove a heavy auto directly over the skeleton, as it afterwards proved, no less than three times, which may well account for a fresh extension of the fracture of the thigh, noticeable when the place was thoroughly uncovered and made clean.

Thus, from the impossibility of putting a dead body into such a position without confining the limbs in some way; and from the testimony of the disassociated neck vertebrae, while elsewhere the normal dropping down of the parts from unhampered decay is plainly seen, it seems safe to conclude that this body was tied up, and that very tightly, when buried. A strong band was evidently passed beneath the knees and over the neck, and probably also the hands were tied at the wrists and the feet at the ankles.

We may get corroboration of this theory, that the corpse was tied up tightly before burial, as well as a very reasonable suggestion as to the purpose, from the paper of Richard Andrée[1] who makes an exhaustive review of the custom of "Hockerbestattung" in all countries, and among peoples of every age, so far as known. He sets aside the embryonal theory, previously touched upon, as too fantastic and improbable to be considered; he treats similarly the idea of this position as that assumed by primitive man in sitting or lying; and he finds little evidence that the folding up is due merely to the desire to get the body in as small a hole as possible, and thus save work for the grave-diggers. He refers to the almost universal idea of the return of the dead to alarm or torment the living, and to the extraordinary means often resorted to to prevent this, and finds here the true explanation of the bound corpse. This suggestion appears to have been first expressed by Schoetensack in the case of the Australians,[2] to which Andrée adds numerous other cases from all parts of the world, and at various periods of time.

[1] *Archiv für Anthropologie*, Bd. VI (1907), pp. 282–307.

[2] *Verhandlungen der Berliner Gesellschaft für Anthropologie* (1901), p. 522.

Thus Strabo reports of the Troglodytes that they bound the neck of the corpse to the legs with withes of thorn, while remains of cords are still to be found in Egyptian graves of the Stone Age. In the island of Ceram, and also in Greenland the people are so anxious to tie up the corpse that they are led to begin operations before death. The same idea, that of not returning to haunt the living, appears in the funeral ritual of many peoples, both with and without the actual binding, and usually takes the form of direct appeal to the body not to come back.

Among some tribes more violent methods than simply tying are resorted to in order to prevent the return; and we find such customs as the smashing of the vertebral column with a heavy stone, as is done in Damaraland, or the burial at the crossroads with a stake of holly through the heart, as has been actually done in recent times in Europe.

In the article of Andrée here referred to is found a rich collection of burial customs in connection with the folding up of bodies for interment; and in all, at least wherever there is any special care used in the position of the body, there runs the thought of so fettering the body in the grave as to prevent a return. This fear may be founded on the idea that the body may avenge its death upon the living; that he may take back with him someone of the family still alive; or perhaps in certain cases, that a demon may get possession of the body and thus be enabled to do grave mischief, of a sort that, in his usual disembodied state, he would be powerless to effect.

The next occasion for obtaining local aboriginal skeletons came to the authors a year after the previous excavation, when, on November 22, 1916, some workmen on the street railway in Greenfield, putting in a spur track beside the power house at the locality known as Cheapside (fig. 58), dug through no less than three aboriginal skeletons, placed in almost an exact row, some twenty to thirty feet apart. But few traces of bones were left when we visited the spot, but there was the usual story about their being "in a sitting position," and one of the workmen went so far as to place himself in the exact position of one of the skeletons as he had observed it.

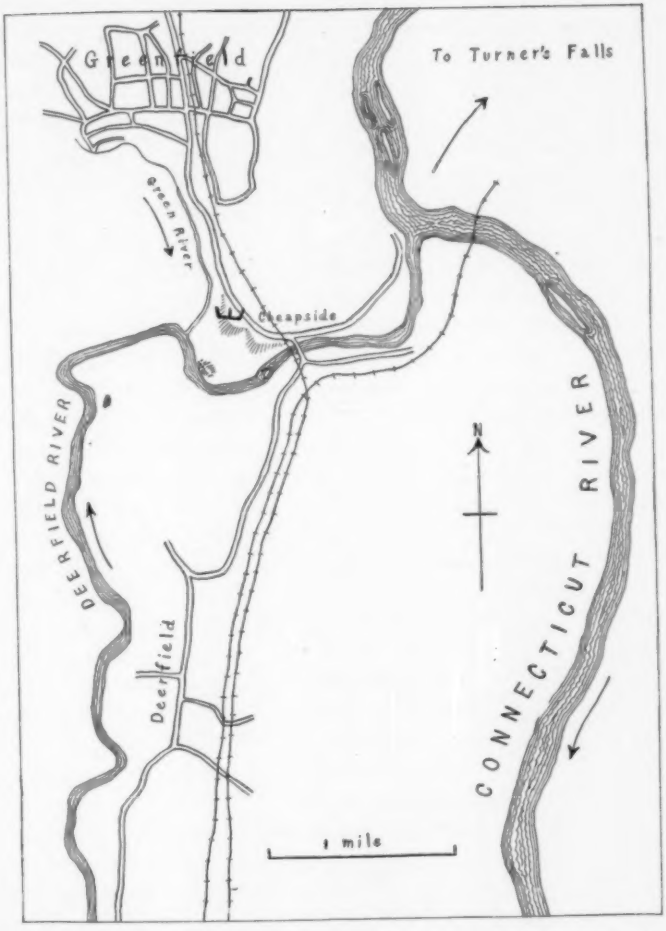

FIG. 58.—Sketch map, showing the locality where the second of the skeletons described in this paper was found. The first occurred at the site given in this periodical, vol. 5, p. 296.

Such could not, however, have been the case, since the entire excavation, from the surface to the level at which the workmen stopped, was but twenty-eight inches, while the mark of one skull was still visible in the frozen soil, the top ten inches below the surface. The stratum which included the entire skeleton could not therefore have been more than eighteen inches thick, quite insufficient to have included a sitting skeleton in the upright position claimed. Moved by this inexorable logic the workman next claimed that the leg bones were still in the earth below the previous excavation, but not only did further careful digging fail to reveal any bones, but did show undisturbed soil, while pieces of both femur and tibia were discovered among the debris left from the first digging. This refutation of the popular claim universally heard locally, that the Indians buried their dead in a sitting position, is extremely important, since, in the two cases of 1905, and the two recorded here, such was certainly not the case. They were sitting, in the sense that they were folded up, with the legs flexed, but were then placed on the right side, and were better described as lying, with the limbs drawn together. In the extremely careful description of Indian burials at Deerfield, the local historian, George Sheldon, describes all the graves the excavation of which he personally witnessed. The skeletons were lying in the flexed position, as in all of our cases, but in all instances cited from the testimony of others, the offending phrase is constantly used. It seems probable from the descriptions that in one or two instances of pretentious interments, where there was a rude stone cist for the reception of the body, the skeletons were really found sitting erect, with the skull on top, but until such a grave is excavated by an experienced archaeologist, and recorded by the aid of photographs, or, better yet, by the permanent preservation of the entire interment, some doubt may be allowed even here. Naturally our failure in finding sitting skeletons in this region does not disprove their presence here, and has nothing to do with interments elsewhere, but thus far, in four cases carefully excavated by ourselves, and in the case of some half-dozen others found by workmen, the skeletons were not sitting, but lying folded up, a position that

could be easily mistaken for a sitting one if the grave were carelessly exhumed.

The place where these Greenfield skeletons were dug up was an ideal site for an Indian cemetery. The entrance of the Green river into the beginning of a U-shaped bend of the Deerfield encloses on three sides a piece of meadow several acres in extent. Along the fourth side runs a slight elevation in the form of a ridge, lying across the meadow, and forming the fourth side of the enclosure. This ridge is rather indefinitely divided into knolls, the westernmost being oval, and semi-distinct, measuring some 30 × 50 feet in diameter. This is the spot where all the skeletons have been found, as it is the only place where any digging has been done about here and of this perhaps two-thirds has already been leveled, resulting in the discovery of about a half-dozen graves. The remainder of the ridge, and a bit of raised ground at the other end of the meadow, almost on the river bank, seem likely spots for further excavation.

So far as can be ascertained there is no tradition connecting Indian activity with this particular knoll, but it lies in the center of activity of the once powerful Pocumtuck, who were the overlords of all the river tribes, Squakheag, Nonotuck, Agawam, etc., between Brattleboro and Hartford, and who in the middle of the seventeenth century, were rated by Gookin at about the same number as the Narragansett. The Dutch in 1652 classed them among the "Great Indians." During the earliest part of the period of English settlement they were at war more than once with Uncas, that questionable protégé of the English, and were in temporary alliance with the Narragansett, and Niantic, to aid them against their Indian foes. In 1664, after treacherously murdering two ambassadors from the Mohawk, who had come to make with them a league of lasting peace, they were attacked by the entire manhood of the offended Mohawk nation and literally wiped out. After this the Pocumtuck disappear from history, but a few wandering individuals of the tribe appear in the English records, generally in those of the court. One old woman, Mashalisk, who owned land on the east side of the river, and who probably owed her preservation to this location, appears as a seller of these lands in payment

for the debts of her son, contracted at Pynchon's trading store in Springfield.

This definite date for the destruction of the Pocumtuck tribe allows us to date the use of this knoll as a cemetery as prior to 1664, and, from the fragile condition of the bones, far more so than in the case of the North Hadley site, it may well be one or more centuries before this time.

The fact of the number of skeletons discovered in removing a part of this knoll showed the strong probability that others are still present in the remaining part, and it is in this connection that we record with great pleasure the great courtesy with which our desire to dig here was met by the superintendent of the street railroad, Mr. John A. Taggart, who not only gave us full permission to excavate as we wished, but on various occasions sent men to assist in certain parts of the work which required aid, especially giving the services of the carpenter of the company, a man of great ingenuity and resourcefulness, who accomplished the boxing up of the grave in a most expeditious manner.[1]

Beginning the excavations by trenches mainly parallel to the excavated edge, we located a skeleton on the second day of work, finding first the upper end of a left femur, and then the associated tibia and fibula. Ascertaining the general position of this leg, we next located the skull, and the left humerus, after which, as in the other case, we surrounded the entire grave with a trench. As an improvement in the technique may be mentioned the fact that the frame that was to serve as the sides of the box was first made apart from the site, and strongly reinforced, then brought to the spot and pushed down to place, and that then the bottom boards were driven through from one side to the other by repeated gentle

[1] While this article was in press we have had occasion to conduct further excavations at this site, and have found three more skeletons. These were in the same position as the one described, and in all are seen the two differences from the position at North Hadley; (1) the legs not so much flexed, and with the femora extended out at nearly a right angle from the body axis, and (2) the erect position of the vertebral column. They were also oriented as the other, with the body axis north and south, and heads to the south, and when located on a plot with the position of the others, so far as could be learned, were seen to run in definite rows placed north and south, the bodies placed lengthwise in the rows, and not across them, as in our cemeteries.

strokes of a heavy sledge hammer. This succession of light jars, contrary to what might be expected, caused less breaking or cracking of the earth than did the steady push of the crowbar used in the first case. Two planks were then placed beneath the whole, across the boards forming the bottom, and the whole was lifted to the surface by eight men. Here it may be said that it is of first importance to have the entire skeleton re-covered by earth, packed down firmly, before the work of boxing or lifting is attempted, and it is equally important to have all the lifting done by a sufficient number of men trained for this sort of thing to avoid any possibility of shock caused by slipping or jerking.

As in the other case, the transportation to the laboratory was accomplished by means of an auto truck, and the subsequent excavation was performed in a similar manner. Here, however, the problem was a more difficult one, as the bones were much less well preserved, and even the strokes of a clothes brush brought away the surface in the form of fine flakes and removed the inner tissues. It was also much less complete, as the bones of the hands and of one foot were entirely gone, leaving no trace even, while of the other foot, the right, there remained only the calcaneum and talus. Here there was no question of lack of care in the excavation, for the work was continued slowly through several weeks, allowing the whole to dry out thoroughly so that much of the earth was removed by blowing. This phenomenon, frequently noticed in old skeletons, is the more remarkable since the distal ends of radius and tibia, adjacent to the hand and foot, are distinct and practically entire; while there immediately ensues nothing but the sand. The skeleton is also entirely gone through the middle of the trunk, the first few thoracic vertebrae being in place and well preserved, while they leave off suddenly, together with their ribs, not to be resumed until the last of the lumbar series is reached, when the bones again appear in place and practically entire. The presence of two isolated ribs located under the left femur, suggests a solution of this serious loss, and gives a picture of some burrowing animal pushing downwards and forwards relative to the skeleton, dislocating the two ribs as found, and either taking out the missing

parts, or so opening up the soil that they soon disappeared through decay. The activity of a carnivorous animal, like a fox, while the body was still fresh is possible, but the space between the leg bones, in the present state at any rate, is hardly sufficient to allow so large an animal to pass far enough to deposit the ribs.

Finally, as to the position of the body, important similarities and important differences are shown in comparison with the other subject of this paper. The general position and orientation, folded limbs, body placed on the right side, and face towards the east, is the same in both, but the parts are not bound so tightly together. The back is straight, without the pulling down of the head and neck so conspicuously seen in the other specimen. The legs and arms, too, are not pulled up as close to the body; the legs are spread somewhat apart, and the left knee is not dropped down to touch the other leg. One hand only, the right, was held before the face; the other lay on the knee of its own side.

In hardening the bones and surface earth in this specimen a new fixing substance was suggested by the junior author, and used here with excellent results, namely, a thin solution of water glass. This formed an especially good coating over the soft and fragmentary surfaces presented by the bones, holding the parts in place, even when almost pulverized, as on the side of the skull. The subject was an old man, with senile jaw and a single tooth, visible in the photograph (fig. 57). It has every appearance of having antedated the one from North Hadley, by a great many years, as both were in similar soil, well drained.

In the above account we have been careful to give the details of our procedures, thinking that they may prove useful to others in other excavations. Since our own experience has been to find better and better ways of doing things with each venture, many improvements of our methods will suggest themselves to anyone who tries an excavation of skeletons *in toto*. Some application of sheet iron or similar thin material for running beneath the grave and forming the bottom of the box will undoubtedly be developed; also materials better than either shellac or water glass may be found for hardening the whole mass, and converting it into

a thin slab more easily handled than a box of dirt. This latter is especially to be desired since present methods do not allow any examination of the soil immediately beneath the body, one of the likely places for associated artifacts.

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RESTORATION OF A CLIFF-DWELLER

By HARRIS HAWTHORNE WILDER

THE accompanying photograph (fig. 59) gives the result of a plastic reconstruction of a skull, by the method given in this magazine some five years ago,[1] but with the addition of the neck and shoulders. These additional parts are built directly on the bones of the same individual, which are fortunately in our possession, and therefore the proportions, although individual, cannot fail to be approximately correct. Their method of restoration differs widely from that of the skull, for while in this latter the soft parts are thin and to a considerable extent rigid and unvarying, we have here to deal with a movable skeleton, covered with muscles, both of which elements vary in relation and actual contour with each movement. It is thus necessary to first place the bones in the desired position, making allowance for capsules and other soft parts at the joints, and then build up the muscles, one at a time, using the muscle scars, processes, and so on as an indication of their position. Interspaces usually found filled with fat may be then filled in with the plastic substance, here plastilina, and finally a fairly uniform coating to represent the skin should be covered over the whole. Although I have never used it, it would seem advantageous to reconstruct the subcutaneous part of some material like clay,[2] which would harden when the work is done, and over which the outer layer could be modelled with more precision.

In the present case the considerable labor of properly adjusting the bones was obviated, since the skeletal parts had dried in their natural position, and had thus never been disassociated. This circumstance saved much trouble, and naturally furnished results that were far more reliable.

[1] *American Anthropologist* (N. S.), vol. 14 (July–Sept., 1912), pp. 415–436.

[2] Permodello, put out by the Prang Co., would probably be very suitable for this work, as it remains soft so long as it is kept moist, but hardens when dry.

FIG. 59.

This method of restoring the soft parts from the muscle scars is not a new one, but has been employed more or less by the paleontologists, notably by Professor Lull in restoring the "frill" about the neck of *Triceratops*. His restoration of the Spy Man,[1] also, naturally took the muscles somewhat into consideration, although it was not built up directly on the bones themselves, as in the present case.

There are certain rather glaring errors in this restoration, due largely to inexperience, which could readily be corrected in another case; the most noticeable being, perhaps, the too high arm pit, the result of a wrong estimation of the breadth of the pectoralis major, as it runs out towards its insertion. The shoulders seem rather too high, but this is probably an individual peculiarity, as it was given by the bones in their natural position. There may be noticed in the photograph a curious twist of the end of the nose, which is apparent rather than real, and due mainly to the way the shadow falls. A careful examination of the light side of this organ shows that it is built out as fully as on the other side, and that, although in fact slightly scoliotic, there is not that drawing up of the right side that appears here.

The special matter of interest, however, and the main reason for the presentation of this paper, is that the woman here restored is the same one whom I figured in this journal thirteen years ago, as restored from the condition of a sun-dried mummy by the use of caustic potash, and it is thus of interest to compare the photograph here presented with those shown in Plate II of volume 6 (Jan.–March, 1904). Naturally there is no especial similarity in the *expression* in the two preparations, since in the former case the lips were wide apart, one eyeball was replaced by cotton, the nose had not been brought out to its full extent, and there was no hair on the head. If one recall the ease with which a good actor may become unrecognizable by simply distorting his face, he can see how fugacious and changeable a thing facial expression is. The main *features*, however, are not very different in the two cases, and the recent restoration may well represent the former one,

[1] *American Journal of Science* (Feb., 1910).

brought to life. In the restored mummy one saw a young middle-aged woman, rather thin than stout, with high shoulders and possibly a slight stoop; who had borne children, and been employed during her lifetime in the hard, active work of aboriginal existence. This sort of person is shown by the new plastic restoration, and, while I naturally had all this in mind while making the restoration, the definite restrictions of anatomical relations and of the measurement of thicknesses were absolutely adhered to, and no unauthorized touch was allowed, save the lines about the mouth and the slight furrowing of the forehead, which were probable, though not definitely indicated.

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IROQUOIAN CLANS AND PHRATRIES

By C. M. BARBEAU

THE Iroquoian race, formerly occupying most of the territory extending from the Great Lakes to the present states of Carolina, consisted of the Cherokee, the Tuscarora, the Five Nations or Iroquois proper, the Huron-Wyandot, the Neutral, and the Erie, with a total population aggregating from eighty thousand to one hundred thousand at the time of the discovery. Each one of these peoples was made up of many smaller nations, divided into tribes, and the tribes into towns and villages with an average population of less than two thousand souls. While political independence was, as a whole, the essential attribute of the village or town, there existed a marked tendency towards wider centralization which, in some cases, resulted in the federation of towns, tribes, and even nations.

Of far greater importance was the Iroquoian kinship organization, the ramifications of which extended far beyond the national frontiers. The only fundamental kinship unit, universal among the Iroquoian and neighboring peoples, is the totemic clan. Other relationship organizations, such as the phratry and the family, were either sporadic or subsidiary. The family, for one, was an ill-defined and amorphous group of close relatives tracing their descent to the same *remembered* and immediate maternal ancestor. There was no marked distinction between different families within the clan, which were supposedly issued from a common remote maternal ancestor. The clan was to the families what the tree is to the limbs. The more interesting relation between the phratry and the clan is the main object of the present study.

The Iroquoian and neighboring Algonkian [1] clans, on the whole, were widely diffused exogamic kinship units, endowed with in-

[1] Under the name "Algonkian" are included the nations speaking an Algonkian dialect.

conspicuous crest and supernatural guardian systems, the membership of which was inherited in the maternal line or conferred through adoption. As will be seen in the following lists, the clans were few in number; but their component parts were found disseminated in sections of numerous towns of many tribes and nations. Distance and linguistic barriers did not prevent the members of the same clan from acknowledging their relationship and mutually abiding by the rules of exogamy and fraternal help.

Of the Cherokee clans little is known besides the fact that they once numbered fourteen, and are now supposed to be "seven clans"—a reference to a mystic number. "The Wolf clan was the largest and most important" (J. Mooney); the Deer, Bird, and Paint clans are the only other ones the English names of which are given.

The number of the Tuscarora clans is not definitely known. Hewitt gives four slightly different lists, in which the order varies, presumably because they referred to different parts of the nation. The clans are: (1) the Bear, which appears the first in every list; (2) the Wolf, divided in one place into Gray Wolf and Yellow Wolf, which is given as second, fifth, and seventh in different lists; (3) the Turtle, twice given as subdivided into Small Turtle and Large Turtle, appearing in the third and fourth place; (4) the Deer clan, to which the third rank is granted in two lists, and the fifth and sixth rank in the two others; (5) the Eel clan, appearing in the second place in three lists, and sixth in the other; (6) the Beaver clan, to which the fourth, fifth, or seventh place is granted; and (7) the Snipe clan, given everywhere as the last. Morgan, on the other hand, states that, when the Tuscarora were admitted, about 1712, into the Iroquois League, their eight "gentes" were arranged into two sides or phratries, as follows:—first phratry: (1) Bear, (2) Beaver, (3) Great Turtle, (4) Eel; second phratry: (1) Gray Wolf, (2) Yellow Wolf, (3) Little Turtle, (4) Snipe. The Deer, he adds, became extinct in modern times.

There was no uniformity in the arrangement of the clans among the Iroquois proper or Five Nations. The Mohawk and the Oneida, the easternmost nations, seem to have had only three

independent clans each, which are given in the following order: (Morgan) the Bear, the Wolf, and the Turtle; (Goldenweiser) the Turtle, the Wolf, and the Bear. On one modern Iroquois reserve, Goldenweiser has found these clans subdivided into three component parts of the same nature. The clans of the Onondaga nation belonged to two phratries. The first phratry consisted of the Wolf, Turtle, Snipe, Beaver, and Ball clans; the second, of the Deer, the Eel, and the Bear (Morgan). The first of the Cayuga phratries claimed the Bear, Wolf, Turtle, Snipe, and Eel clans; the second, the Deer, Beaver, and Hawk clans (Morgan). The Seneca nation, the westernmost one, consisted of the Bear, Wolf, Beaver, and Turtle clans, in the first phratry; and the Deer, Snipe, Heron, and Hawk, in the second (Morgan); or, according to Goldenweiser, of the Turtle, Bear, Wolf, and Ball, in the first phratry; and the Hawk, Deer, Snipe, Duck, and Eel, in the second. The Deer, however, is not found in the modern list.

In a manuscript dated 1666,[1] it is stated that the Iroquoian nation was divided into two bands, respectively of four and five families. The first band was named Gueyniotiteshesgué, which is said to mean four families; and the second, Ouichiniotiteshesgué, *i. e.*, five families. These families (clans) are given as follows: in the first band, 1. the Turtle, 2. the Wolf, 3. the Bear, 4. the Beaver; in the second, 1. the Deer, 2. the *Grand Pluvier* (large plover), 3. the Wild Potatoe, 4. the *Petit Pluvier* (small plover), and 5. the *Kilion* (a bird, possibly the Hawk).

The Wyandot clans seem, at least theoretically, once to have been grouped into two phratries and a third odd unit, the Wolf clan. One phratry consisted of the Big Turtle, Mud (small) Turtle, Prairie Turtle, and Hawk clans; and the other, of the Deer, Bear, Porcupine, and Beaver clans. The Snake and the Snipe clans, moreover, have, in modern times, been added to that list. Informants state that the Snake clan originated out of the Deer clan, with which it is still related. As to the Snipe clan, it was recently introduced from the Cayuga-Seneca system into that of the western

[1] Dominion Archives of Canada, Ottawa, in the *Correspondance Générale*, F 2 (1663–1667), p. 2.

Wyandot. The Lorette Huron claim to have belonged to four clans, the Turtle, the Bear, the Deer, and the Wolf. There is no need of giving here earlier lists, such as those of Finley, Clarke, Walker, Morgan, Powell, and Connelley, which, on the whole, agree with that of the author given above. Some confusion, due to the lack of a careful study, is to be noticed in the case of the many Turtle clans, which, according to Connelley, would be as many as five. The so-called Eagle clan, in Finley, is evidently a misnomer for the Hawk clan. Nothing is known of the clans of the extinct Erie and Neutral nations.

In the various Algonkian, Siouan, Muskhogean, Yuchi, Pueblo, and other lists of clans we find, unevenly represented, most of the Iroquoian clans, besides many others.[1] The diffusion of the same clans here is in all probability due to direct transmission or to imitation.

The Wolf clan seems the most important and widely distributed among the Algonkian, the Iroquoian, the Siouan, and other eastern, southern, and central nations. It is found among the Cherokee, Tuscarora (Gray and Yellow Wolf), Mohawk, Oneida, Onondaga, Cayuga, Seneca, Wyandot, Huron, Mohican, Abenaki, Ojibwa, Potawatomi, Miami, Menomini, Shawnee, Sauk and Fox, Kickapoo, Oto, Missouri, Winnebago, Mandan, Chickasaw, Creek, Yuchi, Hopi, Taos, and presumably many others.

The Bear clan is found among the Tuscarora, Mohawk, Oneida, Onondaga, Cayuga, Seneca, Wyandot, Huron, Mohican, Abenaki, Ojibwa, Potawatomi, Miami, Menomini, Shawnee, Sauk and Fox, Kickapoo, Iowa, Oto, Missouri, Winnebago, Quapaw, Ponca, Kansa, Omaha, Pawnee (?), Creek, Yuchi, Acoma, Hopi, Zuñi, Sia, Taos, Walpi, Nambe, and Pecos.

While the Deer clan is not found among the easternmost Algonkian, it obtains among the Cherokee, Tuscarora, Onondaga, Cayuga, Seneca, Wyandot, Huron, Shawnee, Menomini, Winnebago, Quapaw, Kansa, Omaha, Pawnee (?), Chickasaw, Creek, Yuchi, Hopi, Zuñi, Sia, and Pecos.

The Turtle clan, on the other hand, has a more easterly distribu-

[1] Morgan, *Ancient Society*.

tion than the Deer, namely among the Tuscarora (Small and Large Turtles), Mohawk, Oneida, Onondaga, Cayuga, Seneca, Wyandot (Big, Mud, and Prairie Turtles), Huron, Delaware, Munsee, Mohican (Little, Mud, and Great Turtles), Ojibwa (Mud, Little, and Snapping Turtles), Menomini (Mud Turtle), Shawnee, Quapaw, Omaha, and Yuchi.

The distribution of the following clans is limited to fewer nations. The Beaver clan is found among the Tuscarora, Onondaga, Cayuga, Seneca, Wyandot, Huron, Ojibwa, Abenaki, Potawatomi, Menomini, Oto, Iowa, Pawnee (?), Yuchi, Creek, and Quapaw nations. The snipe clan is found among the Tuscarora, Onondaga, Cayuga, Seneca, Wyandot (recently introduced), Ojibwa, Sauk, and probably many others. The Eel clan has its constituent members among the Tuscarora, Onondaga, Cayuga, Seneca (according to Goldenweiser), and Mohican (Yellow Eel). The Hawk clan is found among the Cayuga, Seneca, Wyandot, Abenaki (Pigeon Hawk), Ojibwa (Pigeon Hawk), Potawatomi (Black Hawk), Menomini, Sauk and Fox, Hopi, and Sia. The Snake clan, recently introduced among the Wyandot, belongs to the Abenaki, Ojibwa (Snake and Water Snake), Shawnee, Iowa, Oto, Missouri, Winnebago, Quapaw, Ponca, Yuchi, Walpi, Pecos, Hopi, and Zuñi. The Porcupine clan is found among the Abenaki, Wyandot, Menomini, and Hopi. The Duck clan belongs to the Seneca (according to Goldenweiser), Ojibwa, Menomini, Hopi, and possibly some other central tribes. The Ball clan seems to belong exclusively to the Onondaga and (according to Goldenweiser) the Seneca. The Heron clan is found among the Seneca, possibly the Mohican, the Ojibwa, Menomini, and some Pueblo tribes. The Wild Potatoe clan, which a 1666 manuscript [1] gives as Iroquois, belongs also to the Sauk, Yuchi, and Creek.

The clans, among some Iroquoian and other nations, were federated into larger exogamous bodies, now termed phratries, the main function of which seems to have been political. The distribution of the phratries by itself indicates that they must have spread gradually and in comparatively recent times among the

[1] Morgan, *ibid.*

few Iroquoian and Algonkian nations who possess them, and that their growth is historically disconnected from that of the clans. Out of eight Iroquoian nations only four surely had a phratric system. These are the Tuscarora, the Onondaga, the Cayuga, and the Seneca. The Mohawk and Oneida clans may possibly have been subdivided into smaller clans, standing in the relation of clans to phratry; but this point has still to be proved. Among the Wyandot, moreover, it seems that once an attempt at least was made to arrange the clans into phratries.

Phratries were also found among some of the neighboring Algonkian nations, such as the Delaware, who had three phratries, as follows: I. Wolf, Bear, Dog, and Opossum; II. Little Turtle, Mud Turtle, Great Turtle, and Yellow Eel; III. Turkey, Crane, and Chicken clans.

The occurrence of the phratries and moieties, although sporadic, is fairly common in the North American tribes and among people whom the Iroquoian tribes are likely once to have come into direct or indirect contact with, such as the Yuchi, the Siouan tribes, the Muskhogean tribes, and the Pueblo and Mexican Indians. It is therefore safe to consider this institution as a derived rather than an original and exclusive one.

Since our subject-matter has a direct bearing on the often discussed history of the phratries and clans, we will now consider some characteristic features in the nature and growth of those of the Iroquoian tribes and their neighbors. L. H. Morgan, in 1877, explained the growth of phratries in the following words:[1]

> It proves conclusively the natural process by which, in course of time, a gens [clan] breaks up into several, and these remain united in a phratric organization, which is expressed by assuming a phratric name. . . . It shows . . . that the phratry is founded upon the kinship of the gentes.

This generally accepted hypothesis on the origin of the American phratries was also applied by A. Lang to some Australian phratries; and Goldenweiser has lately adhered to the same view, expressing his belief in the priority in time of the phratry over the clans

[1] L. H. Morgan, *Ancient Society*, 1907 ed., p. 100.

among the Iroquoian tribes. This point has since been debated. It seems, however, that this hypothesis is untenable, as the clans, and not the phratries, are universal, ancient, and essential among the Iroquoian and neighboring nations.

Our reason for considering the phratric and clan systems, in the Eastern Woodlands, as unrelated to each other in their development may be outlined under the following headings: I, The geographic distribution of the clans and the phratries in North America does not coincide; II, while clans are universal among the Iroquoian tribes and most of their neighbors, phratries appear only sporadically in a minority of Iroquoian nations, and seldom in their immediate vicinity; III, the lack of uniformity in the arrangement of clans in the phratries reveals an arbitrary purpose rather than unconscious growth and transmission; IV, while the clans have never ceased to the present time to be at the basis of the social system, the phratries have disappeared together with the Iroquoian confederacy; V, the totemic attributes of the clans were never shared by the phratries, with the exception of the accidental exogamic rule, which proved to be more persistently a clan than a phratric feature.

I. Eponymous clans seem to be the most typical and widely distributed form of kinship organization in North America. Phratries, on the other hand, cover a much smaller expanse, and their essential traits are not necessarily grafted upon clans or confined to their area of diffusion; but, among many western tribes, they appear in the form of tribal, exogamic, or ceremonial moieties.[1] There is little doubt that, in the course of time, the moiety or phratric system as well as specific eponymous clans spread independently in various directions, from as many unique, but not necessarily identical, centers. While the clans may claim a considerable antiquity in the eastern half of North America, the phratries and moieties are distinctly at home in the southeast, center, southwest, and Mexico, wherefrom the Iroquoian ones must have been derived. It is, moreover, remarkable that wherever

[1] The term moiety is here used to refer to an aggregation of non-totemic social units, whether such aggregations are two or more in number.

the phratric system is wanting as a whole the clans show no tendency towards growing into phratries, in the manner above described.

II. Practically none of the Iroquoian clans is exclusively its own. Wolf, Bear, Deer, and Turtle clans are not only found in every Iroquoian nation, but in a great many tribes from the Atlantic coast to Arizona. The identical three Turtle clans, on the other hand, obtain only among the Ojibwa, Wyandot and Delaware; and two are claimed by the Tuscarora. The obvious centers of diffusion of the Hawk, Snake, Porcupine, Duck and other minor clans lie outside of the Iroquoian frontiers, wherein they were introduced only at one or two places. It is still remembered among the Wyandot, for instance, how and about when the Snake and Snipe clans came into existence; the Snipe clan having branched off from that of the Seneca, and the Snake clan having appeared when they were living in close contact with the Ojibwa, who had a Snake clan.

While to be an Iroquoian Indian it was essential to belong to a clan, it may be said that the phratric system affected only a minority of their population, belonging possibly to less than one half of their nations—that is the Onondaga, Cayuga, Seneca, and Tuscarora. The Cherokee and Huron had no phratries, as far as we know. The three Mohawk and Oneida clans have been considered merely as such. And the actual or prolonged existence of Wyandot phratries is problematical. The Delaware phratries were a strikingly exceptional development among the Algonkians, and were evidently due to Iroquoian influence.

III. Had the most ancient clans of the Iroquoian tribes grown into phratries by gradually splitting up into related constituent clans still retaining their genetic connection, we should now find evidence of this ancient process. The reverse is true, however, as we have seen above.

The arrangement of the clans within the phratries, moreover, does not show any genetic development, as they are found now in one, now in an opposite phratry. The Wolf clan was the head of a phratry among the Onondaga, Tuscarora, and Delaware. It was a subdivision of the phratry headed by the Bear among the Cayuga

and Seneca. In the above-mentioned manuscript list it is given as a subdivision of the group headed by the Turtle. And among the Wyandot and other nations it stood alone. The Bear clan was at the head of a phratry among the Tuscarora, Cayuga, and Seneca; but it came last in the Onondaga phratry under the leadership of the Deer, and second in the Wyandot phratry under the Deer clan and in the Seneca and Iroquois lists of Goldenweiser and the above-mentioned manuscript. It stood independently elsewhere. While the Turtle (or Big Turtle, when there is more than one) was the leader of a Delaware and an hypothetical Wyandot phratry and is also given that rank in the old Iroquois manuscript referred to, it is given after the Wolf and the Bear clans, in the same phratry, among the Tuscarora, Onondaga, Cayuga, and Seneca. The Deer clan, which is almost uniformly given as the head of a phratry among the Onondaga, Cayuga, Seneca, and Wyandot, does not seem to exist in the Mohawk, Oneida, and Tuscarora nations. Although the Hawk clan, when it is found, is of minor rank, it is given by Goldenweiser as the head of a Seneca phratry; and the Deer clan comes only after. The same thorough lack of consistent arrangement—if we consider phratric relationship—is found in the case of every clan, of whatever importance. Notwithstanding the probable fact that the three Turtle clans were derived from one another, two of their subdivisions are found in opposite phratries among the Tuscarora. The Bear and Deer clans, in three nations, are found in the same or corresponding phratry, and in three others in the opposite one. The Wolf clan is either associated with the Turtle clan or is independent. In the two neighboring Seneca and Cayuga nations the Beaver clan is found respectively in the phratry led by the Deer clan and in the opposite phratry. And so on.

If such a state of affairs could never be explained satisfactorily in the light of the contested hypothesis, it becomes logical enough if we assume that the guiding principle in the arrangement of clans into phratries was the deliberate consideration of the balance of power, or the approximately even division of numbers between the two or three phratries, in the national councils or in the ceremonial functions. In other words, it can only be understood through the

assumption that a conscious political federation of clans once took place in several Iroquoian nations, together or severally; such a move being probably fostered by the imitation of a similar foreign example and by the desire of improving the national efficiency through a greater unity in its councils and wars.

IV. Although the few Iroquoian phratries probably existed before the creation of the League or Confederacy (about a century before the discovery), the clearness of their outlines must partly be due to their functional importance in the operation of the Confederacy, which they have followed in its downfall, thus allowing the clans once more to become exclusively supreme, as in their original condition. The only vestiges of the former existence of the phratries in the four Iroquoian nations that once possessed them are to be found in the memory of informants, in myths, and in the more palpable forms of many surviving rituals and ceremonies. The "brother and sister" relationship and connected exogamy once embracing all the clans in the same phratry have long been discarded or forgotten; and, in the course of a statistical investigation on this point, Goldenweiser could only notice a weak tendency among the older people towards avoiding intermarriage between the clans formerly of the same phratry. The only informant possessing the Wyandot tradition about the phratries strongly denied that exogamy ever applied to the phratries, although his recited text plainly implied it. The same text, moreover, describes the creation of the Wyandot phratries as having taken place in a great national council at which several foreign tribes attended.

It should also be remembered that, with the exception of the exogamic taboo, the phratries were operative factors only in the case of a reunion of a national council or of important ceremonials. The respective centers of the various clans were usually scattered far apart, in different towns; and these had to be convoked and represented in order to fulfil their most essential duty. In the absence of councils or ceremonials, the phratries were only latent, and a continued inactivity meant their disappearance. This in itself indicates the nature of their elaborate political origin or federation of naturally dispersed elements.

v. The Iroquoian clans are typically totemic; but this cannot be said of the phratries. While the heraldic and supernatural aspects of Iroquoian totemism were moderately developed in the clans, the phratries did not share these traits in the slightest degree. The clans were eponymous, or named after a certain animal designated in a metaphorical term; but the phratries were nameless, although ethnographers have a tendency, for the sake of convenience, to name them after their head clan. In the 1666 manuscript, as we have seen, the first phratry was termed "the-four-families," and the second "the-five-families," from the fact that there were four clans in one and five in the other. Each clan owned exclusively a list of individual names, a varying proportion of which referred to the clan animal; such a proportion being large among the Wyandot, and trifling—we are told—among the Iroquois proper. Although the clan animal was considered as a supernatural protector of his human confederates, the evidence here is rather scarce, and points to the fact that this totemic trait may originally have been still less marked than the ancient Iroquoian custom of representing pictorically the clan totem on various objects belonging to the clan or its members.[1] As there were no phratric, but only clan, animals, the phratries as such were devoid of all the totemic features which had not been extended to them by the clans. And exogamy, which —we assume—at first was a clan taboo, resumed its earlier form as soon as the phratries dropped out of existence. When citizenship was conferred upon a native child or a stranger it was only through his adoption into a clan; the phratry was not vitally concerned in this process.

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[1] The above-mentioned manuscript states that each family had the picture of its "family" animal painted in the gable of its house, some in black and others in red (p. 395).

PARALLEL BETWEEN THE NORTHWEST COAST AND IROQUOIAN CLANS AND PHRATRIES

By C. M. BARBEAU

THE following brief parallel between the tribal and kinship groups of the Northwest Coast tribes and those of the Iroquoian stock was first meant to accompany the above article, "Iroquoian Clans and Phratries" and a paper entitled "Growth and Federation in the Tsimshian Phratries," published separately in the *International Congress of Americanists*, Washington, 1915.

There is a close similarity only in the village or tribal system in both areas. Although the culture of these peoples was of a different type, their village communities were alike in so far as they were more or less permanent complexes of kinsmen and mutually unrelated people. In other words, they embraced members of different exogamous groups, either families, clans, or phratries. While there is no doubt that, in one or more villages, the members of one clan or phratry were predominant, it is now too late to map out precisely what were the former respective centers of influence of the many Iroquoian clans. Notwithstanding the fact that the village communal life had a certain tendency to create a bond of sympathy between its unrelated members, the kinship ties between relations living in different villages always remained the fundamental factor in their social organization.

When we compare the families, clans, and phratries of the Northwest Coast with those of the Eastern Woodlands, we find some comparable features, though much specialized in different directions, and many striking differences. Among the Tsimshian and neighboring nations of the coast, the phratries, clans, families, and even houses, were totemic units, and were patterned after one another just as if the smaller unit were an offshoot of the larger one. It was quite different among the Iroquoian tribes, where the clan

alone was totemic. The degree of development of the various individual totemic features naturally differed in these two areas. The belief in the supernatural protection and guidance of the group totem was nowhere in the foreground, although present; but while the use of its pictorial or plastic representation as a crest was quite trivial among the Iroquoian tribes, it reached an abnormal development on the West Coast. The lists of individual names belonging exclusively to the clans among the Iroquoian tribes, and either to the phratries, the clans, the families, or the houses, on the Coast, reveal an uneven tendency to refer to some real or mythical traits of the totem; such references being more common on the West Coast. Exogamy was everywhere the main phratric attribute, although, among the Iroquoian tribes, it was derived from the more typical and persistent clan exogamy.

What strikes the observer as the most fundamental fact in the Iroquoian social system, taken as a whole, is the clan. On the Coast, however, the phratries and the families alone are plainly discernible. To get some knowledge of the clans one has to study the history of the people and unravel their complex relationship system. The clan here, in the course of time, gradually loses its individuality in the phratry. The Iroquoian clan, on the other hand, always retained its marked individuality within the phratry, and when one of the two disappeared it was the phratry and not the clan. Contrarily, the downfall of the ancient customs, on the West Coast, has left the phratries firmly established in the mind of the natives, while the frontiers between the clans now have almost disappeared. When, moreover, the neighbors of the Iroquoian tribes borrowed or imitated their social system, the clan was the usual model. But most of the interior neighbors of the Northwest Coast people have borrowed the familiar Coast phratries, presumably without duplicating their constituent elements.

The difference between the phratries in both areas is radical. The Coast phratries are totemic, the Iroquoian ones are non-totemic and purely political. The first grew around a clan nucleus, both through the differentiation of its parts and the numerous gradual outside accessions; the second were due to the arbitrary

and sudden federation of hitherto unrelated clans into an evenly balanced political body. As a consequence, while the Iroquoian phratries were operative chiefly in tribal or national councils and feasts, those of the West Coast invaded every phase of domestic life.

In conclusion, we may mention the perplexing problem of the possible historical relation between the West Coast and the Eastern Woodland phratries through many intermediate stages. Although it is not in our power to find a solution of this question here, we feel that the various phratric and moiety systems, appearing sporadically in many parts of North America, may have had a common remote origin or a single center of diffusion.

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GROWTH OF THE NASAL BRIDGE IN CHILDREN

By LOUIS R. SULLIVAN

THE material used in this investigation was collected by Professor Boas in 1890 in connection with a series of measurements on school children of Worcester, Mass. The other measurements have been treated elsewhere.[1] The following report is in the nature of a laboratory exercise in a course on "Growth" at Columbia University under Professor Franz Boas who planned and supervised the work.

The shape of the nasal arch was recorded by means of a stout lead wire bent across the bridge of the nose from the inner canthus of one eye to that of the other. The cross-section thus obtained was then transferred to paper by tracing. In measuring a specially prepared transparent instrument marked off in millimeter squares was used. The transverse diameter of the cross-section of the bridge of the nose was measured at points 2, 4, 6, and 8 millimeters from the apex. The average for each year, the variability, and number of cases are recorded in Table 1.

TABLE 1

TRANSVERSE DIAMETER OF NASAL BRIDGE AT POINTS 2, 4, 6, AND 8 MM. FROM APEX

(Measurements in mm. a = average for each year. σ = variability. n = number of cases)

Boys

Age in Years	2 mm. from Apex			4 mm. from Apex			6 mm. from Apex			8 mm. from Apex		
	a	σ	n	a	σ	n	a	σ	n	a	σ	n
5	7.6	± .60	5	11.3	± .40	5	15.2	± .80	5	19.8	± .60	4
6	6.8	±1.03	30	10.1	±1.26	30	13.4	±1.91	30	16.8	±2.47	26
7	7.0	±1.01	51	10.1	±1.43	51	13.2	±1.96	51	16.1	±2.76	43
8	7.0	±1.21	45	10.3	±1.53	45	13.2	±2.15	45	16.3	±2.72	42
9	7.0	±1.45	55	10.1	±1.98	55	13.4	±2.66	55	16.5	±3.28	44
10	7.0	±1.00	49	10.2	±1.20	49	13.3	±1.82	49	16.5	±2.19	47
11[2]	—	—	—	—	—	—	—	—	—	—	—	—
12	7.2	±1.13	50	10.1	±1.70	50	13.0	±2.62	50	15.7	±3.61	44
13	6.9	±1.08	46	10.0	±1.52	46	12.7	±2.13	46	15.9	±2.83	44
14	6.8	±1.08	29	9.7	±1.65	29	12.1	±2.55	29	14.3	±3.21	28
15	6.5	±1.13	25	9.5	±1.23	25	12.2	±1.83	25	14.7	±2.59	25
16	6.7	± .97	10	8.9	±1.27	10	10.8	±1.47	10	12.6	±1.60	10

[1] Boas, Franz, and Wissler, Clark, "Statistics of Growth," *Report of the Commissioner of Education for 1904.*

[2] 11 year old boys missing.

Girls

5.....	7.3	± .87	12	11.4	±1.29	12	15.3	±1.64	12	17.5	±1.11	4
6.....	7.2	± .90	20	11.0	±1.40	20	14.9	±2.23	20	16.8	±1.77	8
7.....	6.7	± .88	33	9.9	±1.34	33	13.0	±1.80	33	15.4	±2.40	19
8.....	6.7	±1.02	34	9.9	±1.37	34	12.9	±1.79	34	15.2	±2.21	21
9.....	6.7	± .82	34	10.0	±1.41	34	13.1	±2.12	34	15.5	±2.48	26
10.....	6.9	±1.01	35	10.4	±1.37	35	14.0	±2.14	35	15.2	±2.63	10
11.....	6.7	±1.02	40	10.1	±1.42	40	13.5	±2.61	40	15.9	±2.21	25
12.....	6.6	±1.06	46	9.5	±1.31	46	12.3	±1.94	46	14.3	±1.74	38
13.....	7.1	±1.22	31	10.2	±1.75	31	13.2	±2.18	31	15.9	±2.85	28
14.....	7.1	±1.15	37	10.0	±1.64	37	12.4	±1.80	37	14.8	±2.46	31
15.....	6.7	±1.25	16	9.4	±1.73	16	12.0	±2.37	16	13.5	±2.25	12
16.....	7.1	± .86	5	9.7	± .75	5	11.9	± .58	5	13.5	± .89	5

Inspection of this table shows a gradual decrease of the transverse diameters with increasing age. The irregularity of the decrease is undoubtedly due to the small number of cases recorded and to the inaccuracies inherent in the method adopted.

In order to obtain a clearer picture of the character of growth, we have assumed the transverse diameters to decrease proportionally to time, so that the diameter w_t at the age t may be expressed by the formula

$$w_t = a + bt.$$

This reduction gives the following results:

TABLE II

THEORETICAL VALUES FOR TRANSVERSAL DIAMETERS OF NASAL BRIDGE AT POINTS 2, 4, 6, AND 8 MM. FROM APEX

Girls					Boys				
Age	2 mm.	4 mm.	6 mm.	8 mm.	Age	2 mm.	4 mm.	6 mm.	8 mm.
5.....	6.9	10.6	14.2	16.8	5.....	7.1	10.5	13.6	17.1
6.....	6.9	10.5	14.0	16.5	6.....	7.1	10.4	13.5	16.9
7.....	6.9	10.4	13.8	16.2	7.....	7.0	10.3	13.4	16.7
8.....	6.9	10.3	13.6	16.0	8.....	7.0	10.2	13.3	16.5
9.....	6.9	10.2	13.4	15.7	9.....	7.0	10.2	13.2	16.3
10.....	6.9	10.1	13.2	15.4	10.....	7.0	10.1	13.1	16.1
11.....	6.9	10.0	13.0	15.2	11.....	7.0	10.0	13.0	15.9
12.....	6.9	9.9	12.8	14.9	12.....	7.0	10.0	12.9	15.7
13.....	6.9	9.8	12.7	14.6	13.....	7.0	9.9	12.8	15.5
14.....	6.9	9.7	12.5	14.3	14.....	7.0	9.8	12.7	15.3
15.....	6.9	9.7	12.3	14.1	15.....	7.0	9.8	12.6	15.1
16.....	6.9	9.6	12.1	13.8	16.....	7.0	9.7	12.5	14.9
Decrease...	0	1.0	2.1	3.0	Decrease...	0.1	0.8	1.1	2.2

From Table II we see that there is a gradual decrease in the diameter from year to year. We can only interpret this smaller diameter to mean forward growth of the nose. At birth, in all races, the transverse diameter of the bridge is much greater than the antero-posterior diameter. As growth continues in the Caucasian groups these diameters tend to become equal. The greatest amount of growth is in a forward (antero-posterior) direction. The distance between the canthi of the eyes is probably fixed at a very early age. On account of the absence of definite landmarks, the amount of lateral growth has been disregarded.

The minimum amount of this forward growth may be shown in the following way. Assuming the maximum diameter to be constant, the distance from this diameter to the apex of the bridge will increase and thus the antero-posterior growth may be determined. Assuming the diameter of 13.5 mm. as constant, the following amounts of forward growth are found:

	Boys	Girls
5 years	5.9 mm.	5.6 mm.
6 "	6.0 "	5.7 "
7 "	6.1 "	5.8 "
8 "	6.1 "	5.9 "
9 "	6.2 "	6.0 "
10 "	6.3 "	6.3 "
11 "	6.3 "	6.4 "
12 "	6.4 "	6.6 "
13 "	6.5 "	6.8 "
14 "	6.6 "	7.1 "
15 "	6.7 "	7.3 "
16 "	6.8 "	7.6 "
Forward growth	.9 "	2.0 "

We also notice that in the given time there is a greater decrease in diameters or a greater forward growth among the girls than among the boys. Assuming the form of nose to be the same for both sexes at an early age and a narrow high nose to be a form belonging to older individuals, it may be inferred that the girls for a given age are older physiologically than boys of the same age. Examining the measurements for each year we find that:

5-year-old	girls	correspond	best	with	the	6-year-old	boys					
6	"	"	"	"	"	"	"	9	"	"	"	
7	"	"	"	"	"	"	"	12	"	"	"	
8	"	"	"	"	"	"	"	12	"	"	"	
9	"	"	"	"	"	"	"	12	"	"	"	
10	"	"	"	"	"	"	"	12	"	"	"	
11	"	"	"	"	"	"	"	10	"	"	"	
12	"	"	"	"	"	"	"	14	"	"	"	
13	"	"	"	"	"	"	"	12	"	"	"	
14	"	"	"	"	"	"	"	14	"	"	"	
15	"	"	"	"	"	"	"	14	"	"	"	
16	"	"	"	"	"	"	"	14	"	"	"	

In almost every case the girls correspond to much older boys, but the fourteen, fifteen, and sixteen-year-old girls all correspond to the fourteen-year-old boys. We take this to mean that the girls are practically physiologically mature at fourteen while the boys continue to grow after this age. It is not meant to infer that these numerical data are final, but it is of interest that they agree with what has been found in other cases of growth.

These data suggest that a racial study of the nose should be based on accurate studies of its development during the period of growth. A comparison with the nose of anthropoid apes can be conducted successfully only by the same method. In a thorough study of this problem the form of the entire nose should receive consideration and casts should be substituted for the inaccurate lead wire cross sections. The number of cases should be greatly increased and since the annual growth is so slight the age unit could be increased from 1 to 4 years. A full appreciation of racial differences in anatomical structures must be preceded by a knowledge of the process of growth.

AMERICAN MUSEUM OF NATURAL HISTORY,
NEW YORK CITY

THE SIO SHALAKO AT THE FIRST MESA, JULY 9, 1916

By WALTER HOUGH

With Commentary by Dr. J. Walter Fewkes

THE writer, while engaged in work in Tusayan for the United States National Museum, was present during the Katcina ceremonies and made some notes on one of them, which, though fragmentary, are appended. It is evident that shortly many of the Hopi ceremonies will cease, due to the increasing rapidity with which the influence of the white man is progressing.

The Sichomovi Sio Shalako is a Zuñi ceremony adopted by the Indians of Sichomovi, a village on the East Mesa.[1] The history of the adoption of the ceremony appears to be as follows:

Many years ago a delegation of Hopi went to Zuñi to attend the Shalako with the purpose of introducing it into Sichomovi. They are said to have received help from the Zuñi, but no charge was made. The ceremony is held pursuant to previous announcement given two years before but it has been performed in 1914 and 1916. Extensive preparations were necessary for the 1916 ceremony and as there were only two long rooms in Sichomovi sufficiently large, an addition was made to Connor's house and a new room built on another house. This work and the preparation of the costumes entailed great expense. The performers divided into four Shalako parties prepared at a house in the plain, Ushteoki, or Five Houses as it is called, and progressed slowly with many halts and songs across the valley to the mesa, arriving about seven o'clock in the afternoon. A group of Walpi priests awaited to receive them at the shrine below the mesa, having brought there all sorts of things in readiness, as Koyemshi masks, bundles of soapweed, stalks of new corn, rattles and drums. A row of conducting

[1] The first and so far as I know the only description of the Sio Shalako ceremony at Moki is by Dr. J. Walter Fewkes in the *Fifteenth Annual Report of the Bureau of American Ethnology*, pp. 296–7.

priests made a meal trail on the mesa along their course. One priest, Y'ate, carried a round deep basket, and the others, bags of *homgnumne* or prayer meal. They laid the *puhu*, or a cord with feathers attached, along the meal trail and expectantly awaited the arrival of the procession which they would allow to pass. The Shalako procession was led by Harry, the Snake Chief. First appeared the Fire god, Shulawitzi, an imp of blackness, bearing on his back a rack-shaped bundle of rabbits freshly killed, so that there must have been a rabbit hunt in the valley before the ceremony. Next came eight Shepaquin (Shaiastashas) with feathered and painted helmets with a parrokeet feather bundle atop; bearing in one hand a bunch of yucca leaves and in the other a bunch of cedar boughs. On the left wrist hung a rattle of sheep's scapula. These and all other priests were costumed, so far as can be judged, exactly as in the Zuñi ceremony. Some slight crudities in painting and other matters were noted and it is probable that an observer familiar with the Zuñi procedure would have made note of other variations from the Zuñi ceremony. The awe-inspiring Shalako next appeared, each with an attendant (Hakto) and accompanied with a crowd of Koyemshi as chorus. The Shalako danced four times around the plaza squatting four times each round upon a cloud design in prayer meal. The attendant priests in ordinary costume sprinkled the dancers at intervals with prayer meal and shouted directions. At the conclusion of the dance in the plaza the Shalako each went to the room which had been prepared for his reception. The Koyemshi removed their masks and painting, but the other celebrants retained their costume. The night drew on and a feast was brought in by women and laid on the floor of the ceremonial chamber. It was a bounteous spread consisting in the case observed of ten piles of *piki*, sixty loaves of bread, fifteen washbowls of sheep or goat stew, four buckets or pots of coffee, and some dishes of fruit. Some eighty persons, consisting of Navajo, bystanders, etc., sat down to the feast and there were loud sounds of deglutition. This scene was being repeated in the other ceremonial chambers in Sichomovi.

The chorus, consisting of those who had been Koyemshi, had

gathered and in their midst was a big drum. The dirt floor of the room had been covered with muslin and three huge Rochester hanging lamps furnished the light. Deer skins folded to represent the deer, and with basket tray for head, hung on the wall together with blankets, and an especially large Navajo blanket hung back of the Shalako. Some chairs for guests were arranged on the south or left side; the other side was reserved for the women and children, who also filled the back of the room. The remains of the feast were removed but not before some grasping Navajo had filled bags with *piki* for a rainy day. Much smoking ensued until the guests were becoming impatient. Finally at the tap of the drum and sound of gourd rattles the chorus began. The dancers consisting of two Shalako attendants (Hakto), and two Saiastasha (Shepequin) began. At intervals a pair of dancers disappeared through the door and Koyemshi took their places but the Hakto remained. After a while one of the Hakto crept under the Shalako dress and made the figure move to the dance and gnash its teeth horribly. When guests fell asleep from fatigue the Hakto would strike them with the yucca bundle. The *monkohu* carried by the Hakto was a green rod having four breath feathers attached to it with cotton cords. Their dance kept up all night and in the morning the Shalako danced around the plaza four times and went away to Ushteoki and the Sio Shalako ceremony was over. At least seventy persons took active part in the dance as follows: ten village priests, four Shalako, four Hakto, eight Shepequin, one Shulawitzi or god of fire, and perhaps forty-eight Koyemshi. Dr. J. Walter Fewkes has pointed out the desire of the Hopi to adopt new Katcinas efficacious in bringing blessings. This, in part, explains the adoption of the Zuñi Shalako by the East Mesa Hopi and presents an instance of the transference of ceremonials, as well as giving an opportunity for studying comparative ceremoniology since undoubtedly changes from the Zuñi ritual have been introduced by the fertile genius of the Hopi. The song cycle is a composite of songs learned at Zuñi, songs of the Hopi Shalako, and songs invented for the occasion. The night songs were not counted but I estimated that there were eighty. The Zuñi Shalako number six;

said to represent the four points of the compass and above and below. The Hopi have adopted four in accordance with their quaternary scheme, which seems to have the primacy among this tribe. It is said, also, that the ceremony is announced two years in advance but it was not learned in what manner. The rites concerned with seating the Shalako in the dance room if any were practised were not observed.

Dr. Fewkes kindly drew upon his great store of knowledge of Hopi ceremonial and furnished the following interesting commentary on my observations:

I have read your account of the Sio Shalako at Sichomovi almost literally by the light of my camp fire on the Mesa Verde, and as my time during the day is occupied by strenuous work on the ruin at Mummy Lake, I have little opportunity to comment upon it in a way I might wish to help you. Moreover my notes are all in Washington and I must rely mainly on my memory regarding the ceremony which you have described. I first heard of the Sio Shalako in 1894 and was then informed that it had not been performed for twenty years previously. As I remember the performance and compare it with your manuscript notes, I should judge the ceremony has lost much and gained nothing except by-plays in the interval between the celebration in 1894 and that you witnessed in 1916. I was told that this ceremony was introduced from Zuñi by Pautiwa, a man from Oraibi, who was visiting Zuñi with ten other Hopi. It is interesting to note that no other pueblo of the Hopi, including the native village of Pautiwa, ever presented this ceremony. The reason it is given in Sichomovi is that this pueblo was settled by clans who originally lived in Zuñi, or more accurately speaking, tarried in Zuñi for some time in their migration from the Rio Grande country in early times. Sichomovi is called by the Zuñi, a Zuñi town, on that account; and if you will consult the ritual of this village, published in my papers, you will find that the ceremonial dances there are Zuñi in character and the personators bearing distinctly Zuñi names.

The personators arrayed themselves in a house several miles from the east mesa and marched to the town, costumed, along the old Zuñi trail. This house in the plain is owned by the Badger clan and the four chiefs belong to the Lizard, Badger, Patki and Asa clans or Tewa, in their region. The houses on the Mesa in which the ceremonies were performed belong to the eldest sisters of these four clans.

The ceremony witnessed in 1894 began on July 6, when the chiefs and the majority of the chiefs on the East Mesa assembled in the Badger house in the plain and made prayer sticks or sacrificial offerings of a peculiar shape, numbering over two hundred. The masks used in this ceremony are in the keeping of the owner of this house and here the participants dressed themselves for the ceremony,

which occurred on the following day. At sunset on July 7th a procession composed of the Shalakos and other masked personages marched from this house up the Mesa side to the Badger house in Sichomovi. I have elsewhere described certain episodes of this rite which may be found in my account in the *Fifteenth Annual Report of the Bureau of American Ethnology*. There were several masked personages besides the giants in this procession, the most striking of which was, I think, the personator called the Big Head, an ancestral sun god of the Badger clan. There were complicated rites throughout the nights of July 7 and 8, with performances, elsewhere described, in the plaza of Sichomovi. As I remember them they were much more elaborate than those you described. The ceremony culminated on July 9, or rather about midnight of July 8th.

I recall on July 9 a ceremonial race called the Wawash Katcina, in which two large spruce trees, set upright at different ends of the court, were used. The nature of this ceremonial racing, in which many Katcinas appear, you will find in an account of it elsewhere published. One of the interesting episodes was a droll dance in which one of the Katcinas counts one, two, three, etc., during the racing. There were also a few of the mysterious Yaya or fire shamans, who treated a supposed sick man with charcoal. The Yucca Katcinas afforded much amusement to the assembled crowd by endeavoring to climb one of the spruce-trees. The "Delight Makers," as they are sometimes called, or the Tewa clowns, called Paiakayamu, were out in force. They imitated the disgusting habits of the Zuñi clowns and did unbelievable things, for which they were terribly punished by the Yucca Katcinas. I might go on with many details, but perhaps I have given enough to show the general character of this sensational show. Ordinary decency has perhaps eliminated much which was then performed. From your description I judge that the ceremony has lost its real significance and taken on much of the social character of the Zuñi Shalako in December.

There are one or two things, however, which I will add: First the Zuñi characters predominate for the reason of the kinship of the clans of Sichomovi. The whole ceremony, I believe, was like the Zuñi equivalent from which it was derived, a great sun festival; the Shalako Giants representing the sun birds or sky gods of the four cardinal points. As the Hopi have another greatly different sun festival in December, when the Sun drama is elaborately observed, the Badger clan perform their sun festival in the summer solstice month, June–July. I will not burden you with other suggestions about the meaning of the ceremony, but I have many notes which some time I hope to commit to cold type.

My belief is that Sio Shalako at Sichomovi is one of those pageants or show dances of which there are a few in the Tanoan pueblos on the East Mesa when the sensational rather than the serious intention is paramount. I should not for a moment rank it with Soyaluna, the Hopi celebration of the Sun Return, in Walpi at the December solstice, or with the Snake Dance in August, which are so characteristic of the Hopi ritual. The Hopi visitors to Zuñi were impressed with the showiness of the Shalako and brought it to Sichomovi as a festival to add to their already over-burdened system of show dances to amuse the inhabi-

tants, possibly to attract visitors. Naturally they brought the race and to add to its attractiveness they perform it at the pueblo settled by Zuñi clans. It is in a way analogous to the Butterfly Katcina, as in both of them the serious kiva rites are absent or minimized.

U. S. NATIONAL MUSEUM,
WASHINGTON, D. C.

BOOK REVIEWS

METHODS AND PRINCIPLES

The Belief in God and Immortality. JAMES H. LEUBA. Sherman, French and Company: Boston, 1916. (Price $2.00 net).

Professor Leuba's book presents some most interesting investigations into the status of the belief in God and in immortality at the present time among men of science in the United States. Questionnaires were sent to 1,000 men of the physical and biological sciences listed in *American Men of Science*, to 200 historians from the membership list of the American Historical Association, to 48 teachers of sociology and 149 non-teaching sociologists chosen from the members of the American Sociological Society, and to 107 members of the American Psychological Association. Of the physical scientists 56 per cent. were non-believers in God, of the biological scientists 63 per cent. were non-believers, of the historians there were 52 per cent., of the teaching sociologists 75 per cent., of the non-teaching sociologists 45 per cent., and of the psychologists 76 per cent. By non-believers is meant agnostics, doubters, and disbelievers. The kind of God referred to is one to whom one may pray in expectation of receiving an answer, answer meaning more than the subjective, psychological effect of prayer. The percentages not believing in personal immortality were slightly smaller than the percentages not believing in God among all the groups of thinkers except possibly the psychologists. The higher percentages of non-believers among the psychologists, sociologists, and biologists is interpreted by Professor Leuba as being due to the fact that these groups are more accustomed to thinking of natural law applying to mental phenomena as well as to the phenomena of the physical sciences.

Another interesting feature of the investigation was the division of the groups into lesser and greater thinkers and a comparison of the non-believers in each division. In all groups the percentage of non-believers among the greater thinkers was considerably larger than among the lesser thinkers. Thus among the greater biologists 83 per cent. were non-believers in God while among the lesser biologists only 61 per cent. were non-believers. The differences between the lesser and greater thinkers of other groups were approximately the same as between the biologists, save in the case of the historians, where the believers among

the lesser historians were about twice the number of believers among the greater historians. However, I think that the differences between the lesser and the greater thinkers should be interpreted only in general as showing more belief among the lesser thinkers than among the greater; and they should not be compared one with the other because the methods of separating the lesser from the greater cannot be shown to be the same in each case. Of the physical and biological scientists taken together 1 out of every 5.5 of the original lists were called greater, among the historians 1 out of every 3.7, and among the psychologists 1 out of every 3.8. The author does not give the figures from which I could tell what the proportions were for the physical scientists, the biologists or the sociologists. But even if the ratios were the same for each group, the original lists from which the greater were chosen cannot be shown to be sufficiently comparable.

An appreciation of Professor Leuba's very important and interesting investigations involves two considerations, one the framing of the questionnaire, and the other the statistical treatment of the results. The difficulties of framing a questionnaire on this subject are very great. The chief difficulties concerned the words "belief," "God," and "immortality." The reviewer thinks that the author has succeeded admirably in making questions on so complex a subject clear and simple; although it is a fact that the philosophers to whom the questionnaire was sent were unable to understand what the questions meant, or claimed that the terms were not adequately defined.

Concerning the statistical treatment criticisms should be directed at the method of sampling and the size of the samples. Professor Leuba picked his cases at random and gave excellent consideration to the very few who did not reply. There is, however, some fault to find with the size of some of his samples and with some of the conclusions he draws, although most of them and certainly his larger conclusions are thoroughly established. He chose 1,000 names from the 5,500 names in *American Men of Science* and to show that this sample is large enough he divides this list of 1,000 names into two divisions of 500 each and as the results tally for each of the divisions, the conclusion is that his sample is large enough. And he remarks (page 277):

> If, in the case of the scientists we may take the statistics of 1,000 as representative of 5,500 we may, a fortiori, accept the other statistics as representing the whole of each group, since in each the proportion upon which the investigation bears is larger than in the case of the scientists.

Since 1 in every 5.5 scientists is chosen, then if another group had,

say, 1,100 names he would choose 200 (which is 1 in 5.5) as representative. By the same logic if another group had, say, 11 names he would choose 2 as representative. This is surely not the theory of errors. If there should be in the United States (or in the world) only 11 men in a particular science, this 11, although all there are, is yet a sample of a theoretically larger number.

This lack of use of the theory of errors leads the author into several minor conclusions not justified, I think. For instance, he stresses the fact that the psychologists are the only group where the percentage of believers in immortality (19.8 per cent.) is less than the percentage of believers in God (24.2 per cent.), explaining the conclusion by the peculiar nature of the science of psychology. But inasmuch as the sample included only 91 (or 93) replies, one questions if the difference is significant. Working out the error of the difference between the two percentages by the formula

$$\epsilon = \sqrt{\frac{p_1 q_1}{n_1} + \frac{p_2 q_2}{n_2}},$$

where the p's are the percentages, the error is 6 per cent. while the difference between the two percentages is only 4 per cent. In other words the chances of his conclusions being wrong are about 1 in 4. Also other differences between groups where the numbers are small should be interpreted cautiously by the reader.

It was impossible to test by certain statistical methods some of his conclusions, because unfortunately exact numbers are not given. It is decidedly to be regretted that full tables were not given. The author has endeavored, and successfully, to make his book popular and readable. But such tables could have been placed in the appendix. An illustration of this absence is noted particularly in his discussion of the very important question of the correlation between desire and belief in immortality, on which subjects questions were included in the questionnaire. The general result was that a good many more expressed a desire for immortality than expressed the belief and in some instances nearly all who had the belief had the desire. But only in a few cases does he give percentages and in only one case does he give numbers. In that one case I figured the correlation coefficient between desire and immortality to be 0.6, a fairly high correlation, working it out in the usual way, for the four-fold table. Methods of contingency and correlation could have been applied to advantage in several cases. I should repeat, however, that his more important conclusions are quite well established, and in nearly all instances his samples are large enough.

The author conducted two other investigations, among college students, which show that larger percentages of women are believers than men, that larger percentages of lower-classmen are believers than upper-classmen, and that from 40 to 45 per cent. of students leaving college are non-believers.

Not all of Professor Leuba's book deals with the statistics of belief. In Part I he discusses the origin and evolution of the idea of immortality and in Part III he discusses the present utility of the belief in personal immortality, which is held to be very small. These two parts certainly help greatly to give rich meaning to the very interesting statistics. They are also contributions to the study of immortality but not so clearly established by facts as the conclusions drawn from his statistics.

The belief in immortality (or continuation) appears among primitive peoples, the author holds, because of "the exteriorization of vivid memory images, the sense of presence, dreams and the visions of waking life," and is an "unavoidable interpretation, devoid of any moral significance, of facts directly perceived." This primitive belief does not evolve into the modern belief. There is a striking absence of continuity. The modern belief has a separate origin and is the creation of desire, a desire for the realization of moral ideas.

His treatment of the theory, it seems to me, establishes the existence of two important psychological factors determining the belief in life after death and the nature of that life, but neglects the importance of desire among primitive peoples, and magnifies the difference between the primitive idea and those of moderns. He states (page 42): "There is no evidence that the savage ever concerns himself with his own fate after death" and (page 123) that the primitive view of life after death is "devoid of any moral significance" and that "there is no likeness (between the primitive and modern views) whatsoever beyond the mere idea of continuation (of life after death)." In the light of these statements it is well to consider, as one instance, the beliefs of the Central Eskimo. They have a belief in a heaven, where there is no snow and ice and many deer, which is the reward of goodness and a belief in a hell where certain individuals go and are specifically punished by the deity for misdeeds done on earth. That desire among primitive peoples was abundantly translated into other than existing conditions seems true from the interpretations of myths by the psycho-analytic school of psychologists.

A full understanding of the nature of the belief in life after death among any one people at any one time demands a fuller consideration

of a number of other factors than the two the author describes so well, as for instance, the general level of knowledge, the conditions of general religious belief, the conceptions of justice, the social and economic conditions shaping the desires for improved conditions in this life, in the form of a national kingdom or social reform, and perhaps many others. If one thinks of a belief in immortality as shaped by specific factors, the lack of continuity (if indeed this be proven) between primitive views and modern views breaking a certain evolutionary progress, does not assume such theoretical importance.

WILLIAM F. OGBURN

Arboreal Man. F. WOOD JONES, M.B., D.Sc., Professor of Anatomy in the University of London. Longmans, Green and Co.: New York, 1916. Pp. x, 230, 81 figs. (Price $2.40 net.)

Recent literature on the evolution of man has dealt chiefly with the changes brought about by the assumption of the upright gait. In *Arboreal Man* the effects of the erect posture have been a secondary consideration as compared to the influence of early life among the trees.

Evolution is assumed as a fact and all attempts at explaining the methods by which changes of structure came into being, were perfected, and handed down, are wisely disclaimed at the outset. The author defines his position by saying that change has been effected somehow, and somehow such changes have been inherited. Nevertheless if one does not partially accept the principle of "use and disuse" the arguments lose much of their force. Dr. Jones says (p. 6):

> Man comes from an arboreal stock. Two questions arise. When in the phylogeny of Mammals did this stock become arboreal and when did it give rise to a creature which we could possibly term human? The first question is capable of an approximate solution, the second is unanswerable, but we may say with regard to it that, if the term "humanoid" may be permitted, such a stock may have had a very early representation among the mammalian fauna.

As used and undefined by the author the term "humanoid" can lead only to confusion. If we regard it as a synonym of "primate" the answer will probably pass unchallenged. If, on the other hand, we regard it as a synonym of "anthropoid" or, as seems more justifiable, we take it to mean the more immediate ancestors of man, many paleontologists and anatomists will take exception. While the early separation of man from the remainder of the anthropoid stock is always a possibility, paleontological evidence at present does not justify an earlier separation than Miocene.

Mammals which become terrestrial sacrifice the amphibian and reptilian mobility of the limbs for stability in order to gain support for the body and facility of locomotion. This mobility once lost is never fully regained. Much is made of these facts in answering the question, when in the phylogeny of mammals did the "humanoid" stock become arboreal? In the anthropoid apes and more especially in man, we have the retention of this primitive mobility in the greatest degree. Only a very early arboreal life could prevent this member from becoming sacrificed to stability. Accordingly the ancestry of man is taken back to the Cynodont Therapsids (Reptiles) of the Triassic and Permian of South Africa. Here a scene comparable to that enacted by the climbing tailed Amphibians or Tree Frogs is depicted.

That the forelimb of Man and the Anthropoids represents a primitive condition in respect to mobility, number of digits, power of supination and rotation most anatomists will grant. But the view that for this reason, or any other reasons so far brought forward, the ancestry of man, in exclusion of the rest of the mammalia, must be pushed back into the very "dawn" of mammalian evolution will meet with some opposition. The alternative is that the generalized ancestor of all Metatherian and Eutherian mammals was very probably arboreal or sub-arboreal and had just these same characters of mobility of the arm and fore-arm as seen in Man and the Anthropoids. There is much evidence in support of this view. Adopting this theory we avoid the necessity for early separation of the Anthropoids from the remainder of the Mammals and Man from the Anthropoids. Nor do some of us see the necessity of deriving the generalized Mammal from an amphibious form as described above.

While demonstrating the anatomical primitiveness of the human forelimb Dr. Jones takes occasion to decry the ever present tendency to insinuate that in man we find the culminating form of evolution and perfected type. In reference to the brain this is true but in regard to other anatomical structures, such as limbs, the horse may justly claim this distinction.

One of the best chapters in the book is that on the "Human Foot" (p. 73). Some of the praise which has been lavished on the hand is transferred to the foot.

> If man should wish to point with pride to any organ the structure of which definitely severs him from all the other existing Primates, it is to the foot that he should point. If missing links are to be tracked successfully, the foot, far more than the skull or teeth will mark him as the Monkey or the Man.

It is most certainly true that this phase of Anthropology has been subordinated, perhaps necessarily. Man inherited an arboreal foot in which the middle digit was probably longest. Arboreal life had developed the hallux as a grasping member. Since becoming terrestrial man has paralleled the Horse in progressing towards a stage with a greatly reduced pes. That the small toe is a fast degenerating member no one can doubt. It is also true that the central axis of the foot as determined by the grouping of the interossei muscles has changed from the middle to the second digit. That such a change is possible is shown by the partly terrestrial Gorilla in which the central axis is variable, being sometimes the second and sometimes the third digit.

Hand feeding and the preparation of food have successfully conditioned the retraction of the snout region and a reduction in dentition. The newly acquired chin is regarded as a retrogressive character in modern man. If this can be conclusively proven it will be the final blow to the much doubted association of a certain type of chin with speech or a highly differentiated genio-glossus muscle.

One or more chapters are devoted to each organ or system of organs. Arboreal life has left its stamp on the vertebrae, pelvis, viscera and especially the respiratory system. The difficulties of locomotion in such a habitat has reduced the number of offspring among Primates. For those who are inclined to interpret Embryology in the terms of Phylogeny there is an interesting chapter on the "Human Baby" (p. 201).

One of the closing chapters deals with the failures of arboreal life. Among these are the Lemurs and Sloths, which became too highly adapted to arboreal life to change. Man's salvation and success are due to the fact that he was early arboreal but did not become a highly specialized arboreal form.

It is not to be expected in the present state of our knowledge that anyone would wholly agree with Dr. Jones and in some few cases it is to be doubted whether he would get a majority to agree with him. Taken as a whole, it is one of the best zoological treatments of man that has appeared. One thing especially commends it. It deals with evolution as a present-day process and not wholly confined to the past ages. The author's ability as a teacher is shown by the remarkably clear and interesting presentation of some complicated anatomical problems. It is in every sense a readable book and none of the evidences advanced can be passed over lightly.

LOUIS R. SULLIVAN

Rest Days: A Study in Early Law and Morality. HUTTON WEBSTER. The Macmillan Co.: New York, 1916. (Price $3.00.)

The accounts in this book of market days, of lunar days and calendars, of holy days, of evil days and of a miscellany of days under taboo the general reader who is curious about his own holiday observances will find enlightening. The student will find the compilations of facts and of critical conclusions and the full bibliographical notes of convenience.

Were the student to expect anything more, he would be disappointed. No systematic attempt is made to ascertain relations between the data given and other cultural facts, for example, between economic or religious facts. Ethnological and psychological assertions are made, to be sure, but between the assertion and its context there is little or no connection. It may be true, for example, that "the connection of the rest day with the farmers' pursuits is secondary rather than direct" (p. 102), but the pages which follow on market days supply no proof of that generalization. The author is inclined to assume that the belief in death pollution is earlier than the belief in death spirits (p. 63), but he does not say why. Nor does he say why he assumes that memorial rites for the dead or exorcising rites are "massed and assigned to particular times" to meet "a demand for order and precision" (p. 75). For one of his assertions or assumptions he explicitly promises evidence.

> It is reasonable to suppose that, with the deepening sense of social solidarity, observances once confined to the individual alone, or to his immediate connections, would often pass over into rites performed by the community at large. Some evidence tending to substantiate this opinion will be presented incidentally as the investigation proceeds (p. 7).

The promised evidence proves to be further assumption (pp. 25, 38–9, 63).

Other generalizations occur which are not even illustrated by facts. For example,

> The institution of taboo, in its individualistic aspects, has helped to nurse in man a sense of reverence, a power of self-restraint greatly needed under primitive conditions (p. 60).

We might almost infer from this passage, not to speak of others, that Dr. Webster had paid no attention to herd psychology and that it had never occurred to him that a sense of reverence might be in itself a primitive condition.

Consideration of the tabooed day as an expression of gregariousness Dr. Webster might well have found profitable. We should like to know,

for example, what connection there is, if any, between communal rest days and communal activities. But the only way to ascertain the existence of such a connection or the lack of it would be not by psychological assertion, but by the statistical case method or something like it, by the Tylor-Steinmetz method of noting the concomitance in every culture of the facts in question or the lack of concomitance. The method of illustrating a generalization favored by Dr. Webster and others leaves us just where we were.

That Dr. Webster is a devout believer in cultural evolution he leaves us at no time in any doubt. His naïve use of the term "superstition" is the flag he waves incessantly in our face. But perhaps in no passage is his conviction as fully expressed as in the following:

> The ancient dwellers in the Arabian wilderness, who celebrated new moon and full moon as seasons of abstinence and rest, little dreamed that in their senseless custom lay the roots of a social institution, which, on the whole, has contributed to human welfare in past ages and promises an even greater measure of benefit to humanity in all future times (p. 246).

Unless, may we comment, the whole psychosis of crisis, of the meaning of which Dr. Webster seems unaware, should as a cultural manifestation go entirely into the discard. (Will magic, another psychological factor in rest days, modern culture has already fairly well discarded.)

But it is development, not discard, which appeals to Dr. Webster. He is one to whom, as he puts it,

> nothing is more interesting than the contemplation of that unconscious though beneficent process which has converted institutions, based partly or wholly on a belief in the imaginary and the supernatural, into institutions resting on the rock of reason and promoting human welfare (p. 307).

Who will say that the teleologist is a less happy man than the theologist—or a less fatuous?

ELSIE CLEWS PARSONS

El Hombre Fósil, HUGO OBERMAIER. Junta Para Ampliacion de Estudios e Investigaciones Cientificas. Madrid, 1916. Pp. XIV, 397. Numerous illustrations.

Dr. Obermaier is already well known to American anthropologists and prehistoric archaeologists as the author of *Der Mensch der Vorzeit*,[1] of which the present work might be considered as a revised and somewhat abridged Spanish edition. *El Hombre Fósil* is published as Memoir

[1] For review of this work, see *Current Anthropological Literature*, vol. II, pp. 131–138, 1913.

Number IX of the *Comision de Investigationes Paleontologicas y Prehistoricas;* and in adapting his manuscript to the Spanish, the author has been fortunate in the collaboration of Ismael de Pan and Pablo Wernert. In the arrangement of subject matter, in bibliography and

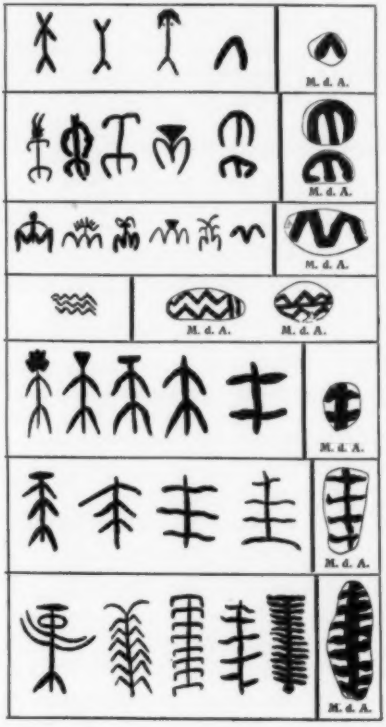

FIG. 60.—Comparison of Spanish petroglyphs with the designs on the painted pebbles of Mas d'Azil. Those to the left of the broad vertical bars are Spanish. *After Obermaier.*

in the scope of the index, the present volume is superior to Professor Obermaier's earlier work. In the opinion of the reviewer, however, the index should have been presented as a unit instead of being divided into three parts.

One of the special merits of the volume is the fulness with which

the latest discoveries have been discussed. The array of evidence in favor of a close correlation between the Spanish petroglyphs and the Azilian culture of France as represented in the painted pebbles of Mas d'Azil is a distinct contribution (fig. 60). The figures to the right of the broad vertical bars are from the painted pebbles, while those to the left are painted on the walls of Spanish caves and rock shelters. A number of colored plates add to the value and attractiveness of the volume.

GEORGE GRANT MACCURDY

Socio-Anthropometry. B. L. STEVENSON. Richard G. Badger, Boston: 1916. 153 pp.

The author of this little volume seems to labor under the impression that there is a considerable school, which she terms the Socio-Anthropometric, which teaches that if the laws of evolution hold in the physical or biological sense they also hold in the psycho-physical; hence, that a certain disposition, or characteristic type of mind, goes with a certain stature, head form, or color. She proposes to subject these teachings to a critical test to see if there is

> correspondence between likenesses and unlikenesses of sociologic and anthropometric types which can lead to the assumption that sociologic facts can be classified according to the postulates of physical anthropometry.
>
> Sociology postulates differences among men. Anthropology postulates differences among men. These differences are illustrated racially.

To test the case, she undertakes to classify the sociological and anthropological traits of "the three European races" and to apply the one to the other. The Teutonic, the Alpine, and Mediterranean races are the subjects of this discussion, but

> because general sociological and anthropological considerations in themselves are too extensive, national units are *ipso facto* prerequisite;

hence, England and Scandinavia are chosen to represent the Teutonic race, France and Russia the Alpine, and Italy the Mediterranean.

In the first group the author finds that two dominant strains have long worked to bring about the two-sided English character.

> The Saxon strain is expressed in love of home, of agriculture, of obedience to law and the democratic principle. The Norman strain shows itself in love of personal liberty, of adventure, of fine quality and truthful virtue, and the autocratic principle.
>
> The English mind is critically intellectual, the English disposition domineering and creative. The rationally conscientious attitude is the mould of the Briton.

The Scandinavians "belong to the category of morality." They express their strongest individuality in the spheres of religion and social rights, are rationalistic but dogmatic. The habit of thought is inductive. The type combines an austere character with a domineering but at the same time convivial disposition. In certain respects—such as their form of religion, in the nature of their government, their love of independence, self-reliance, and allegiance to the principles of logic—these two branches of the Teutonic race approach each other closely, but in other ways they are sociologically very far apart.

Referring to the findings of anthropometry, we are told that in some districts of Scandinavia broad heads, dark colors, and short stature prevail; that there has been mixture with Lapps in Norway, with Finns in Sweden, yet there is in Scandinavia, as a whole, "a racial dolichocephaly, tall stature, and fair coloring."

England's population is more consistently Teutonic in head index, and height, yet even here the traits of various peoples who have contributed to the population are still noticeable. In color, it is found that brunettes make up 31 per cent. of the population; stature varies from 156 cm. to 183 cm. (masculine), and head indices from 72 to 85; yet taken in large numbers they—the English—belong with the Scandinavians to the Teutonic race. To sum up:

> We can say that although physically they agree in type, sociologically they are very far apart in their expression of that type.

The two representatives of the Alpine race—Russia and France—are next considered. The Russian mind is found to be adaptive, imitative, and keenly perceptive, but lacking in will power. It is a logical mind of the deductive variety. Keenness and lucidity of thought predominate, at times to the exclusion of creative ability. The French are convivial in character and creative in disposition. Will power is manifest. The rationalism of the Teuton is absent. Intellectually they are deductive but critical in attitude.

> Sociologically the French and Russians are alike and unalike.

Turning to physical types, the author concludes that these two nations are rightly considered exponents of the Alpine, or Central European, type since in this respect—as well as in ethnologic and sociologic manifestations—"there are to be observed more similarities than dissimilarities." This despite the fact that Russia is "formed of Slavic, Asiatic, and Teutonic strains," that "in France more than in any of the countries instanced, except perhaps Russia, heterogeneity prevails," that the

head index varies in France from 79 to 91.34, and stature from 154 cm. to 177 cm.

The Mediterranean race—represented by Italy—is, sociologically, a highly complex unit, due to the many elements which have contributed to its formation. The disposition is aggressive and instigative, the character a compound of two blends—austere and convivial—yet it is the dogmatic emotional which predominates in the larger character, a cool appraising quality of Italian judgment combined with hot impulsiveness of emotion. It is in the sphere of emotion, morals, and intellect that Italy has won her great sociological achievements.

Physically, Italy, like the countries already studied, shows great variation. In the north the Italian tends to be broad-headed, tall and fair; in the south he is long-headed, short and very dark.

Thus it is seen that the anthropometric character of Italy's population presents a rather regularly culminating geographical order of color, height and head index, which *in toto* gives the general Mediterranean figure with its dolichocephalic head, its short stature and dark coloring.

From the foregoing the author is convinced that there are in Europe three distinct anthropologic types, but only two sociologic types,

since the Alpine and Mediterranean types are not sharply differentiated from one another sociologically,

hence, she concludes that

since neither racial nor national sociologic types hold consistently to anthropometric classification, the assumption is made that socio-anthropometry in postulating certain mental traits combined *sine qua non* with physical traits reduces to an absurdity.

The book appears to be an attack on a definite school of thought, but in order to establish this school, the author has, by implication at least, classed together all those who believe in physical, social or mental evolution; those who believe that environment and economic conditions affect physical man; those who believe in superior races and those who deny such superiority. Thus she classes Vacher de Lapogue, who holds the blond dolichocephal to be a member of a superior race, with Professor Boas, who consistently argues against the division of peoples into inferior and superior. It is true, Professor Boas recognizes that actual differences in races may force us to believe in some differences in their mental makeup; but he has also shown that there is no necessary correlation between race, language, and culture.

To the reviewer, it appears that the author has carefully constructed

a "bogey figure," but out of such diverse materials that the finished article fails to resemble anything existing under heaven or on earth.

Even if we grant, for the moment, that her Socio-Anthropometric school does exist, we fail to see that the arguments here brought forward would invalidate its claims. What the author has done is to prove, by her carefully compiled physical data, that the five nations considered are made up of such heterogeneous types that it would be absolutely unsafe to try to correlate any known set of national mental traits with an average of the physical features. To take the physical average of a population as diverse as that of Italy, and to assign to it the Italian mental traits, as given by the author, is to place in the same category the tall brachycephals of Lombardy and Pedmont, and the short dolichocephals of Lucca and Sardinia. Such a procedure is no more justified than it would be to apply the same traits to the Perm group in eastern Russia, or to a considerable proportion of the population of Denmark.

Finally, the mental or sociological characteristics given for the various nations seem quite as open to question as is the average of physical data. Any argument which assigns the same mental traits to the Englishman of Yorkshire and the Irishman of Kerry needs no further refutation.

FAY-COOPER COLE

NORTH AMERICA

The Constitution of The Five Nations. ARTHUR C. PARKER. (New York State Museum Bulletin, No. 184, Albany, N. Y.: April 1, 1916).

Traditional History of The Confederacy of The Six Nations. DUNCAN CAMPBELL SCOTT, F.R.S.C. (Royal Society of Canada, Proceedings and Transactions, 3rd Series, vol. v., Ottawa, Canada, 1912, Section II, 195–246 pp.).

Civil, Religious and Mourning Councils and Ceremonies of Adoption of the New York Indians. REV. WILLIAM M. BEAUCHAMP. (New York State Museum Bulletin, No. 113, Albany, N. Y., June, 1907).

These three publications are considered here together because they deal with a common topic—the League of the Iroquois. They severally repeat old errors and so diffuse them broadcast under the patronage of learned institutions, and so the following strictures are made on the untrustworthy character of much of their contents, lest the unwary student be led into accepting misinformation for truth.

It must be noted that the second publication also forms a part of the contents of the first, in which the fact of its separate publication by Mr. Duncan Campbell Scott is not mentioned by the editor.

The third publication is interesting chiefly as compilation of texts, notes, comments and quotations rather than as a serious study and analysis of the complex institutions mentioned in the title of the work. Thus, for example, a long list of etymologies is quoted from Morgan, although a large majority of them are worthless; the texts and translations cited from Hale are not revised and corrected, although in very many instances they are misleading or erroneous. The want of accurate knowledge of the languages of the peoples to which the work refers made Dr. Beauchamp the victim of the palpable blunders in translation which his chief informant, the Rev. Albert Cusick, was prone to make.

Mr. Parker tells us that two main manuscripts form the basis of his publication. He fails, however, to point out the value of either manuscript, or to explain the significance of the serious conflict of statements of essential facts or events between the two; we should have been told the essential fact that the document prepared by the Committee of Chiefs of the Six Nations was prepared as a substitute for the Newhouse document, which the chiefs in council had thrice rejected as faulty in arrangement and erroneous or spurious in many of its statements. We are told that these two manuscripts were "discovered" in 1910 on the Six Nations reserve, Ontario, Canada; it is however a fact that the Newhouse "Constitution," although in much briefer form, had been known since 1880, for in that year a copy of it had been left by its compiler on the Cattaraugus reservation for safe-keeping; the document of the chiefs of the Six Nations was prepared in the early spring of 1900 while the present writer was a guest of the late chief John Arthur Gibson, one of the Committee of Chiefs.

Again, Mr. Parker should have explained also that this document of the Committee of Chiefs, supplemented, however, by a portion of the native matter appearing in Hale's *The Iroquois Book of Rites*, 1883, had been published as early as 1912, by Duncan C. Scott, F.R.S.C., in the *Proceedings and Transactions of the Royal Society of Canada* as noted above.

The following comments deal chiefly with the contents of Bulletin 184, by Mr. Parker. The statement that Rev. Albert Cusick was

> employed for more than a month in correcting the Newhouse manuscript until he believed the form in which it is now presented fairly correct and at least as accurate as a free translation could be made

is contrary to the facts. The Newhouse manuscript has appeared in a number of varying versions, which were one and all originally recorded in the English language. But in 1897–8 the present writer induced Mr. Newhouse to undertake with him the translation of the best of these,

after recasting and rewriting certain portions of the various sections. This work was undertaken in order to preserve in Mohawk terms of juridical, governmental, and of ritualistic import, but not for publication in the material as found. So the matter of this document is in no accepted sense a "free translation" of a native text. It is indeed unfortunate that the translated matter quoted by both Dr. Beauchamp and Mr. Duncan from Hale's *Iroquois Book of Rites* should have been used without radical and essential corrections in the forms and translations of vital portions of the Ritual, for the renderings of entire sections are faulty and misleading, and often quite contrary to the intent of the originals.

These editors were apparently quite unaware of the serious blunders in translation and statement they were unconsciously diffusing as sources of further error. A specimen of the untrustworthy character of much of the material in question may be cited here. The provision for a private visit of condolence and sympathy to the death-lodge three days after the burial the body of a dead chief (Duncan, *op. cit.*, Parker, *op. cit.*, 109) is to the point; this visit is to be made by a delegation from the "cousin" sisterhood of tribes, for the purpose of comforting the bereaved family and kindred with the substance of *eleven* (not *thirteen*) of the Fourteen Themes (Ne" Adondakshah) of the Requickening Address; what follows is intended to be the caption of this hearthside address and is in the words following:

> The beginning of the Condolence Ceremony used immediately after the death of a chief (or Lord) and which is subsequently followed by the thirteen ceremony called 'At the Wood's Edge.'

It is clear that the thirteen sections of the address cited here are for the formal public function and so are not at all in the form suitable for use at the private lodge hearthside. Besides there is no such thing as a thirteen Ceremony called "At the Wood's Edge." Mr. Parker quotes eleven strings of wampum, although he cites thirteen sections just as does Mr. Duncan; but the interpolated remarks in section 3, to wit:

> The foregoing part of the Condoling Ceremony is to be performed outside of the place of meeting. Then the bereaved will appoint two of their chief warriors to conduct the four brothers into the place of meeting,

should have been a loud hint to the editors that they were quoting wrong matter. This is a confusion of a private visit with a public function. Careless proofreading permitted an undue number of inexcusable misprints to appear. Skanawatih's (9 and 13) or Skanawita's (30) is evi-

dently printed for Dekanawida's. Abbreviations share in these errors of proofreading; *SPW* (pp. 52, 53, 54 and elsewhere) evidently should be *LPW*. Too much credence is placed in the authenticity of the so-called Passamaquoddy wampum traditions, which are of course concerned with the activities of the "Iroquois" settled at Caughnawaga and elsewhere on the St. Lawrence, Canada, and so have little or no bearing on the early history of the "Five Nations" of New York.

The number of federal chiefs was not fixed at fifty. This is an unhistorical number which is known only within the last century; it arose from a misinterpreted tradition concerning the episode in which one Bearfoot was a chief figure. The first traditionally authentic number is 47, to which in later times were added two Seneca chiefs, making 49 as the highest authentic roll of official titles of federal chiefs. Authentic tradition is silent as to the original number. It is usually found by adding together the several tribal lists.

It is not traditional nor historical to say that for "many generations" the knowledge of "each law or regulation" of the League had been "preserved" by means of "a collection of wampum belts and strings," as the traditions published along with this statement clearly show, for these inform us that the founders of the League knew apparently little, if anything symbolic, about wampum, but rather something definite about "elderberry twigs" and "quills." Hence, the further statement (p. 8) that "Several of the wampum belts in the New York State Museum are Constitutional belts or memorials" seems indefensible, if it is desired to suggest that they were used by the founders of the League. Dr. Beauchamp (*New York State Museum Bulletin*, No. 41, vol. 8) has reached conclusions in accord with this remark.

The statements on page 13 concerning the use of the word "Longhouse" are based on misinformation and superficial observation. In no Iroquois tongue does the native name of the League signify "Longhouse." There is, therefore, never any confusion between the native names for the "Long House," the usual place of assembly, and that for the institution, called the League or Confederation. Trustworthy and discriminating interpreters and informants would so translate these native terms as to emphasize this important difference. The native name of the League is *Gano*^{n}*'syoñ'ni'* (with initial K in some dialects) and signifies "The Extended Lodge," *i. e.*, the Lodge that is Extensive; that is, spread out far, especially lengthwise. But the native name for the ordinary public assembly lodge is *Gano*^{n}*''ses* (in some dialects the last *e* becomes an *i*) and means "The Long Lodge," commonly shortened

to "Long-house"; and so there is no reason for confusion here. So it is a gratuitous remark to say that Handsome Lake destroyed "the old religious system," for there is nothing to show its truth; the great religious festivals, all antedating the time of Handsome Lake, are today still in vogue on the Grand River reservation and elsewhere.

The references (p. 14 and elsewhere) to the "Crooked Tongues" are due to hazy ideas about the facts in the matter. There is no historical or traditional evidence, known to the present writer, showing that the Neuter Nation, so-called, ever had lands lying northeast of Lake Ontario, or that a Huron village called Kahanayen was situated on them. The land in question was probably Huron territory for Dekanawida's date. Evidently, the name "Crooked Tongues" is used as a substitute translation of the Huron and Neuter name Attiwendaronk (Hatiwendaronk) which signifies "Their speech is awry," *i. e.*, "They speak a language slightly different (*from the norm of ours*)." The source of the utter confusion of names and places probably arose from misunderstanding certain information which the present writer many years ago gave to Mr. Newhouse concerning the early inhabitants of what is now Ontario, Canada. This information contained the suggestion that Dekanawida was very probably a naturalized Huron captive among the Iroquois. Singularly, the Huron tribes do not figure in the traditions relating the events leading to the formation of the League. So the comments on page 15 concerning Dekanawida's troubles with his own people are probably fiction, and especially so is the alleged conversation carried on with Mohawk people; the Mohawk did not know the Wyandot (Huron) as the "Crooked Tongues," as the statement on page 14 implies. After crossing the Lake, Dekanawida was not in Mohawk hunting ground, but in that of the Oneida or Onondaga, being a long distance from the Mohawk villages.

There is also confusion between an alleged "immutability" in contradistinction from the asserted "continuity" of the institutions of the League. Amendments to already existing laws are monotonously frequent in the traditions. Again, it is not true that the term *oñgwe'hoñwe'* implies any notion of peculiar "originality" of descent or of "superiority" of race. This compound term signifies "native man," and is a limitation of the general term *oñ'gwe'*, "man-being," *i. e.*, any living being having human attributes, the man of myth, to the "native" strictly *human* man. The Indian knew no race other than his own, for this term is also applicable to the Eskimo. Any other view is untenable.

There is no justification for the substitution of the words, "the soft,

white, feathery down of the globe thistle," for the original false translation, "The Great Belt of White Wampum," of the native term, Jonodakenrahkowa, of the Newhouse manuscript of 1898, for these terms in no sense correct the incorrect rendering and they wholly miss the expressive symbolism of the native expression. The unhistorical character of the list of fifteen "original" clans, appearing in section 42 on page 42, in which seven are spurious, is clear to any careful student of the early clans of the Five Nations, for even one of the most important—the Wolf—has been suppressed without comment. The last three probably owe their existence to *otosis*—originating in the mishearing of names. The so-called "Ball" clan is a Hawk clan; the Opposite-Side-of-the-Hand is a Wolf clan; and the "Wild Potatoe" is a "Tuber Duck" clan; or is possibly due to the mishearing of a dialectic pronunciation of the name for Plover.

The Mohawk text first published by Hale in his *Iroquois Book of Rites*, then by Rev. Dr. Beauchamp, and finally by Dr. Scott (*op. cit.*, p. 238) gives no warrant for the astonishing statements (*Bulletin*, No. 184, pp. 27–28) concerning the clan towns, namely, "Now the party passed through these places" and "All these places are in the Mohawk country." The native text already mentioned states that some of these towns belong to the Wolf clan, some to the two Turtle clans, and some to the Bear clan. It would have been thus unnecessary for the 'party' to march through these towns, for they were evidently not all in "the Mohawk country" but dispersed among all the tribes there represented; and the clans-people present are severally addressed as coming from these several towns, and it is further stated, and this is important, that these *four* clans made up the number of clans "in ancient times." The last statement bears on what has been said about the clan list in section 42.

In article 63 the words "two sons," which occur several times, represent a wrong translation of a native term of relationship which signifies, in this place, "parent and offspring," usually translated, "Father and Son," or "Mother and Daughter." Section 19 is scarcely more than an expansion of section 18, and its provision for an independent "Council of War Chiefs" is unhistorical; too many councils of coördinate jurisdiction would result from it. Sections 55, 56 and 57 are confused and so in their present form unhistorical. The alleged provision for the dissolution of the League is indefensible; the provisions of Sections 56 and 57 are inconsistent one with another, and these in turn are traversed by the ordinances set forth in the second paragraph on page 103 (which is a part of the Committee's document).

The song set forth in Section 105 is certainly not what its caption, "The Installation Song," represents it to be; there is, indeed, no such song. This song was personal to the first Wathatotarho (Adodarho), and so it is not an installation song of to-day. The first line should have been translated, "I possess a fine thing," instead of "It is good, indeed."

On page 91 the statement is made that circumcision was practised on one of the founders of the League. This statement of course is inaccurate, as the Iroquois performed no such rite. The native words have quite a different and symbolic meaning.

The expressions, "white lion," "fire dragon of discord," and "white panther," on page 103, are attempts at translating native words which together are the name of one of the primal man-beings of Iroquoian myths of creation who was therein the personification of Discord. The literal meaning of his name is "the white-bodied meteor or flying-dragon." He brought about discord in heaven (the sky-world) which resulted in the complete metamorphosis of beings. So to translate his native name by the words "white lion" is erroneous, for it does not express the ideas intended in the text. The original manuscript in 1900 contained the words "white lion" at this point. So the present writer pointed out that such a rendering was inaccurate because the early Iroquois did not know the lion but did know the meteor. And he suggested further that as the "Master or God of Discord" was here intended, the better expression in English would be "the white-bodied meteor, the white-bodied fire-dragon or panther, of discord"; the suggested correction was approved by Chief Abram Charles and the late Chief John Arthur Gibson of the Canadian Six Nations, to whom it was made. The native term in note 3 on page 103 signifies "Death, or the Destroyer," a very different idea from the one sought to be expressed in the text.

In regard to the ownership of land, the latter part of Section 42 contradicts Section 44. The two main documents (compare pp. 11 and 12 with 98 and 47, respectively) disagree flatly as to usage in color symbolism. And both are in error in regard to the meaning of the native term "*Ska-no-dah-ken-rah-ko-wah*," which signifies "the very great white mat (foundation)," for both erroneously translate "belt" where they should render "mat." They differ entirely as to its color; the Committee's statement being the correct one. The statement of the Committee's document (p. 103, 2d paragraph) in regard to the community of hunting-grounds is correct; but the Newhouse assertion (p. 45, Sec. 57) is of course inaccurate, because it limits the right to the "one bowl" con-

taining "a beaver's tail" (*i. e.*, the common game preserve) to the "Lords of the Confederacy." His use of the term "cooked" beaver's tail, shows clearly that he has still to learn the meaning of this wise and benevolent provision of the founders of the League.

Sections 99 to 104 do not belong in a work of this kind. And Sections 105 and 106 are wrongly labeled and are not a part of the Constitution of the League. Section 107 does not belong here; many things naturally were taken for granted as well-known common law. Sections 108 to 117, private "Funeral Addresses," do not form any part of the Constitution of the League, and so are out of place here; their crudity and naïveté should have excluded them from consideration.

Section 64 is certainly not a law or ordinance; Section 72 is merely an expansion of 71; Section 73 is contradicted by 57; Sections 19, 59 and 98 deal with the same matters and so in their present form are unhistorical, and so is the last part of Section 79. Sections 74 and 77 are unauthentic; 87 is largely a duplicate of 83; and Sections 82 and 89 are not parts of the "Constitution," so-called. And Sections 85 and 88 are entirely contrary to the basic principles of the League as founded by Dekanawida. Sections 93 to 96, having been translated from Lafitau's *Moeurs, etc.*, by the present writer for Mr. Newhouse, are not traditionally parts of the "Constitution."

Of the footnotes on the pages from 65 to 107, thirty-three are erroneous and misleading.

It is noteworthy that the Secretaries of the Committee of Chiefs of the Six Nations Council admit that the traditions which they recorded have been "much modified" by several causes. But these annalists failed to detect in some notable instances the elements which have been assimilated by the League traditions from their mythic and other tales. Such, for example, are the following: the notion of "the white stone canoe" or the "marble canoe," and the "Ohsinoh" incidents. Now, the "stone" or "flint" canoe belongs to the cycle of stories which relate to the Winter God whose means of travel on water is a block of ice, which is poetically transformed into a "canoe." So this episode does not belong to the Dekanawida legend. Mr. J. V. H. Clarke (*Onondaga*, I, 1849) records the Dekanawida story, but he writes "*white* canoe" only; the original Dekanawida canoe was probably a birchbark canoe. But tradition has expanded "white" into "white stone" as suggested above. Moreover, Clarke mentions one *Ho-see-noke* as "A kind hearted, merry chief" who in behalf of the Council comforted the vexed mind of Hiawatha; but it is found that the Newhouse story makes this man whose

name is in slightly different spelling, thus Ohsinoh (Osinoh) page 18, "a famous shaman" who destroys Hiawatha's daughters by "evil magic arts." Here we find a complete transformation of a man and his deeds.

It is to be noted that an extra verse has been unwarrantably added to the well-known "Six Songs" (Duncan, 239 p.), which derive their common name from the fact that just "six" verses constitute this chant; it is also quite erroneous to say, following Hale, "the Hymn called 'Hail,'" for this designation rests on a mistranslation of the native name and on a worse misapprehension of the real import of the chant.

Other misprints are Jiknosahseh (p. 91), Djikonsase (p. 90), and Djikonsă'se' (p. 71), for Djigo^{n}'să''sē^{n}'. It may be said here that there is no evidence that this person, the so-called Peace Woman, was in any sense a character "in Iroquois mythology." An examination of the provisions of Section 91 shows that they are in serious conflict with those of Section 59.

The inept and wholly whimsical comments on the pictographs published on page 111 indicate that here the editor was the dupe of cocksure but ignorant informants. The utterly fanciful character of these comments is indeed emphasized by the remarkable fact that this page of pictographs and comments appears as an inset in a misnamed and badly garbled summary of the "Re-Quickening Address of the League Ritual of Condolence and Installation." These pictographs from number 4 refer serially to the paragraphs of the summary beginning with number 1 on page 110, although the editor seems unaware of this interesting fact. It is to be noted that this *Address* deals with the *tribes* of the League but not with the *clans* of the League; the printed comments are incorrect in this respect.

The two pictographic groups of parallel lines respectively refer to the 'Father' and 'Mother' side of the League structure—the four representing the 'Mother' side and the three, the 'Father' side; the four lines represent the Oneida, the Cayuga, the Tuscarora, and the Delaware, the so-called 'Four Brothers'; and the three lines, to the Mohawk, the Seneca, and the Onondaga, the so-called 'Three-Brothers.' The prostrate figure indicates that the 'Three Brothers' are the mourning side, and the first erect figure shows that the 'Four Brothers' are the celebrant side according to the Ritual and so are not mourning in the ritualistic sense,—a needful distinction.

Then the pictographs marked 4, 5, and 6 represent the three Acts or Words spoken "At the Wood's Edge," the key words being respectively "wipe away the tears," "clear out the ears," and "remove the

choking from the throat"; these acts are mentioned in paragraphs marked 1, 2, and 2, on page 110. Figure 7 (4 on p. 110) does not hold a "sun" as here stated, but "a cup," containing "the waters of pity"; figure 8 (5 on p. 110) does not denote "a bench with four legs," but rather the "seat" or "the reed mat," said to be stained with blood; figure 9 (6 on p. 112) denotes "the darkness" of grief; figure 10 (7 on p. 112) denotes "the loss of the sky" from grief; figure 11 (8 on p. 112) denotes "the loss of the sun" from grief; figure 12 (9 on p. 112) denotes "the grave," *i. e.*, "the upturned earth"; figure 13 (idea not on p. 112) denotes "20 strings" as the price exacted for a homicide, which is the circle of protection for the two groups of parallel lines; figure 14 (11 on p. 112) denotes "the reverence due the person of the woman"; figure 15 (wanting on p. 112) denotes "the malific powers of the earth"; figure 16 (wanting on p. 113) denotes "the obligation of mutual respect and service"; figure 17 (wanting on p. 113) denotes "the torch of announcement, or of notification"; and figure 18 (wanting on p. 113) denotes "the doorway," *i. e.*, the "end of the address." These brief strictures show how much real harm is done by the rush to publish unstudied material, no matter by whom.

But to enumerate the redundancies, the contradictions, and the misconceptions in Mr. Parker's *Bulletin* would require a volume larger than the publication in question. It is most unfortunate for the cause of historical truth that great institutions insist on publication at the expense of study and accuracy. It may be mentioned that this publication of Mr. Parker has been most unfavorably reviewed by Dr. Goldenweiser in volume 18, no. 3, pp. 431–436, of *The American Anthropologist*. I have purposely not given out this unfavorable estimate of Mr. Parker's recent work until it had been reviewed by one whose motive Mr. Parker might not question.

J. N. B. Hewitt

SOME NEW PUBLICATIONS

Barrett, S. A. The Washo Indians. (Bulletin of the Public Museum of the City of Milwaukee, vol. 2, no. 1, May 10, 1917); pp. 1–52, pls. 1–13.

Bates, Oric, *editor*, and Sterns, F. H., *assistant editor*. Harvard African Studies, 1: Varia Africana, 1. The African Dept. of the Peabody Museum of Harvard University, Cambridge, Mass., 1917. 271 pp., 2 + 18 + 1 + 7 + 3 + 1 + 26 + 1 pls., numerous figs.

—— Ancient Egyptian Fishing. (Harvard African Studies, 1; pp. 199–271, pls. 1–26.)

Cole, Mabel Cook. Philippine Folk Tales. A. C. McClurg and Company: 1916. xv, 218 pp., 28 ills. Price $1.25.

Ellwood, Charles A. An Introduction to Social Psychology. D. Appleton and Company: New York and London, 1917. xiii, 343, pp. Price $2.00 net.

Furlong, Charles Wellington. Some Effects of Environment on the Fuegian Tribes. (The Geographical Review, 1917, vol. iii, no. 1, pp. 1–15.)

Gifford, Edward Winslow. Miwok Myths (University of California Publications in American Archaeology and Ethnology, vol. 12, no. 8, May 11, 1917), pp. 283–338, plate 6.

Kroeber, A. L. California Kinship Systems (University of California Publications in American Archaeology and Ethnology, vol. 12, no. 9, May 28, 1917), pp. 339–396, 1 sketch map.

Laufer, Berthold. Burkhan (Journal of the American Oriental Society, vol. xxxvi, 1917, pp. 390–395.)

—— The Beginnings of Porcelain in China. With a Technical Report by H. W. Nichols. (Field Museum of Natural History, Publication 192, Anthropological Series, vol. xv, no. 2, pp. 79–183, 12 pls., 2 figs.) Chicago, 1917.

Lowie, Robert H. Plains Indian Age-Societies: Historical and Comparative Summary. (Anthropological Papers of the American Museum of Natural History, vol. xi, part xiii, pp. 877–1031.) December, 1916.

Means, Philip Ainsworth. A Survey of Ancient Peruvian Art (Transactions of the Connecticut Academy of Arts and Sciences, vol. 21, pp. 315–442, 17 pls., 1 sketch map.) Yale University Press: New Haven, Conn., 1917.

—— History of the Spanish Conquest of Yucatan and of the Itzas. (Papers of the Peabody Museum of American Archæology and Ethnology, Harvard University, vol. vii.) Cambridge, Mass., 1917. xv, 206 pp., 6 pls. (including map).

Michelson, Truman. Remarks on American Indian Languages, a Study in Method. (Journal of the Washington Academy of Sciences, vol. vii, no. 8, April 19, 1917, pp. 222–234.)

—— Remarks on Terms of Relationship. (Journal of the Washington Academy of Sciences, vol. vii, no. 7, April 4, 1917, pp. 181–184.)

Morice, A. G. Essai sur l'origine des Dénés de l'Amérique du Nord. (Extrait de la Nouvelle France, Quebec, 1915.) Chez l'Auteur: Saint-Boniface, Man. 1916. 345 pp., 12 ills.

Nelson, N. C. Archæology of Mammoth Cave and Vicinity: a Preliminary Report. (Proceedings of the National Academy of Sciences, vol. 3, pp. 192–195, March, 1917.)

Spier, Leslie. Zuñi Chronology. (Proceedings of the National Academy of Sciences, vol. 3, pp. 280–283, April, 1917.)

Sterns, F. H. The Paleoliths of the Eastern Desert. (Harvard African Studies, 1, Cambridge, 1917, pp. 48–82, 1 map, XVIII pls.)

Wallis, W. D. The Development of the Human Chin. (The Anatomical Record, vol. 12, 1917, pp. 315–328.)

DISCUSSION AND CORRESPONDENCE

Do We Need a "Superorganic"?

Nothing irritates a student of culture more than to have the methods of the exact sciences flaunted in his face as a salutary antidote to his own supposedly slipshod methods. He feels that he deals with an entirely different order of phenomena, that direct comparison between the two groups of disciplines is to be ruled out of court. It is some such irritation that seems to have served as the emotional impetus of Dr. Kroeber's very interesting discussion of "The Superorganic" (pp. 163–213 of this volume). Many anthropologists will be disposed to sympathize with him and to rejoice that he has squarely taken up the cudgel for a rigidly historical and anti-biological interpretation of culture. His analysis of the essential difference between organic heredity and social tradition is surely sound in the main, though doubts suggest themselves on special points in this part of the discussion. The common fallacy of confounding the cultural advancement of a group with the potential or inherent intellectual power of its individual members is also clearly exposed. There is little in Dr. Kroeber's general standpoint and specific statements that I should be disposed to quarrel with. Yet I feel that on at least two points of considerable theoretical importance he has allowed himself to go further than he is warranted in going. I suspect that he may to some extent have been the victim of a too rigidly classificatory or abstractionist tendency.

In the first place, I believe that Dr. Kroeber greatly overshoots the mark in his complete elimination of the peculiar influence of individuals on the course of history, even if by that term is understood culture history, the history of social activities with practically no reference to biographical data as such. All individuals tend to impress themselves on their social environment and, though generally to an infinitesimal degree, to make their individuality count in the direction taken by the never-ceasing flux that the form and content of social activity are inevitably subject to. It is true that the content of an individual's mind is so overwhelmingly moulded by the social traditions to which he is heir that the purely individual contribution of even markedly original minds is apt to seem swamped in the whole of culture. Further-

more the dead level of compromise necessitated by the clashing of thousands of wills, few of them of compelling potency, tends to sink the social importance of any one of them into insignificance. All this is true in the main. And yet it is always the individual that really thinks and acts and dreams and revolts. Those of his thoughts, acts, dreams, and rebellions that somehow contribute in sensible degree to the modification or retention of the mass of typical reactions called culture we term social data; the rest, though they do not, psychologically considered, in the least differ from these, we term individual and pass by as of no historical or social moment. It is highly important to note that the differentiation of these two types of reaction is essentially arbitrary, resting, as it does, entirely on a principle of selection. The selection depends on the adoption of a scale of values. Needless to say, the threshold of the social (or historical) *versus* the individual shifts according to the philosophy of the evaluator or interpreter. I find it utterly inconceivable to draw a sharp and eternally valid dividing line between them. Clearly, then, "individual" reactions constantly spill over into and lend color to "social" reactions.

Under these circumstances how is it possible for the social to escape the impress of at least certain individualities? It seems to me that it requires a social determinism amounting to a religion to deny to individuals all directive power, all culture-moulding influence. Is it conceivable, for instance, that the dramatic events that we summarize under the heading of the Napoleonic Period and which are inextricably bound up with the personality of Napoleon are a matter of indifference from the point of view of the political, economic, and social development of Europe during that period and since? Would the administration of the law in New Orleans be what it now is if there had not existed a certain individual of obscure origin who hailed from Corsica? It goes without saying that in this, as in similar cases, the determining influence of specific personalities is, as a rule, grossly exaggerated by the average historian; but a tendency to deprecate too great an insistence on the individual as such is not the same thing as the attempt to eliminate him as a cultural factor altogether. Shrewdly enough, Dr. Kroeber chooses his examples from the realm of inventions and scientific theories. Here it is relatively easy to justify a sweeping social determinism in view of a certain general inevitability in the course of the acquirement of knowledge. This inevitability, however, does not altogether reside, as Dr. Kroeber seems to imply, in a social "force" but, to a very large extent, in the fixity, conceptually speaking, of the objective world. This fixity

forms the sharpest of predetermined grooves for the unfolding of man's knowledge. Had he occupied himself more with the religious, philosophic, aesthetic, and crudely volitional activities and tendencies of man, I believe that Dr. Kroeber's case for the non-cultural significance of the individual would have been a far more difficult one to make. No matter how much we minimize exaggerated claims, I fail to see how we can deny a determining and, in some cases, even extraordinarily determining cultural influence to a large number of outstanding personalities. With all due reverence for social science, I would not even hesitate to say that many a momentous cultural development or tendency, particularly in the religious and aesthetic spheres, is at last analysis a partial function or remote consequence of the temperamental peculiarities of a significant personality. As the social units grow larger and larger, the probabilities of the occurrence of striking and influential personalities grow vastly. Hence it is that the determining influence of individuals is more easily demonstrated in the higher than in the lower levels of culture. One has only to think seriously of what such personalities as Aristotle, Jesus, Mahomet, Shakespeare, Goethe, Beethoven mean in the history of culture to hesitate to commit oneself to a completely non-individualistic interpretation of history. I do not believe for a moment that such personalities are merely the cat's-paws of general cultural drifts. No doubt much, perhaps even the greater part, of what history associates with their names is merely an individually colored version of what they found ready to hand in their social, philosophic, religious, or aesthetic milieu, *but not entirely*. If such an interpretation of the significance of the individual introduces a repugnant element of "accident" into the history of culture, so much the worse for the social scientists who fear "accident."

The second point in Dr. Kroeber's essay that I find myself compelled to take exception to concerns his interpretation of the nature of social phenomena. If I understand him rightly, he predicates a certain social "force" whose gradual unfolding is manifested in the sequence of socially significant phenomena we call history. The social is builded out of the organic, but is not entirely resolvable into it, hence it implies the presence of an unknown principle which transcends the organic, just as the organic, while similarly builded out of the inorganic, is not resolvable into it but harbors a new and distinctive force that works itself out in organic phenomena. I consider the analogy a false one. Moreover, I do not believe that Dr. Kroeber has rightly seized upon the true nature of the opposition between history and non-historical science.

The analogy is a false one because, while the organic can be demonstrated to consist objectively of the inorganic plus an increment of obscure origin and nature, the social is merely a certain philosophically arbitrary but humanly immensely significant *selection* out of the total mass of phenomena ideally resolvable into inorganic, organic, and psychic processes. The social is but a name for those reactions or types of reaction that depend for their perpetuation on a cumulative technique of transference, that known as social inheritance. This technique, however, does not depend for its operation on any specifically new "force," but, as far as we can tell at present, merely implies a heightening of psychic factors. No doubt the growth of self-consciousness is largely involved in the gradual building up of this technique of social transference. While we may not be able to define satisfactorily the precise nature of self-consciousness or trace its genesis, it is certainly no more mysterious a development in the history of mind than earlier stages in this most obscure of all evolutions. In short, its appearance involves no new force, merely a refinement and complication of an earlier force or of earlier forces. Hence social activities, which I define as a selected group of reactions dependent at last analysis on the growth of self-consciousness, do not result from the coming into being of a new objective principle of being. The differential characteristic of social science lies thus entirely in a modulus of values, not in an accession of irresolvably distinct subject matter. There seems to be a chasm between the organic and the inorganic which only the rigid mechanists pretend to be able to bridge. There seems to be an unbridgeable chasm, in immediacy of experience, between the organic and the psychic, despite the undeniable correlations between the two. Dr. Kroeber denies this *en passant*, but neither his nor my philosophy of the nature of mind is properly germane to the subject under discussion. Between the psychic and the social there is no chasm in the above sense at all. The break lies entirely in the principle of selection that respectively animates the two groups of sciences. Social science is not psychology, not because it studies the resultants of a superpsychic or superorganic force, but because its terms are differently demarcated.

At this point I begin to fear misunderstanding. It might almost appear that I considered, with certain psychological students of culture, the fundamental problem of social science to consist of the resolution of the social into the psychic, of the unraveling of the tangled web of psychology that may be thought to underlie social phenomena. This conception of social science I have as much abhorrence of Dr. Kroeber as

There may be room for a "social psychology," but it is neither an historical nor a social science. It is merely a kind of psychology, of somewhat uncertain credentials, for the present; at any rate, it is, like individual psychology, a conceptual science. It is quite true that the phenomena of social science, as claimed by Dr. Kroeber, are irresolvable into the terms of psychology or organic science, but this irresolvability is not, as Dr. Kroeber seems to imply, a conceptual one. *It is an experiential one.* This type of irresolvability is *toto caelo* distinct from that which separates the psychic and the organic or the organic and the inorganic, where we are confronted by true conceptual incommensurables.

What I mean by "experiential irresolvability" is something that meets us at every turn. I shall attempt to illustrate it by an example from a totally different science. Few sciences are so clearly defined as regards scope as geology. It would ordinarily be classed as a natural science. Aside from palaeontology, which we may eliminate, it does entirely without the concepts of the social, psychic, or organic. It is, then, a well-defined science of purely inorganic subject matter. As such it is conceptually resolvable, if we carry our reductions far enough, into the more fundamental sciences of physics and chemistry. But no amount of conceptual synthesis of the phenomena we call chemical or physical would, in the absence of previous experience, enable us to construct a science of geology. This science depends for its *raison d'être* on a series of unique experiences, directly sensed or inferred, clustering about an entity, the earth, which from the conceptual standpoint of physics is as absurdly accidental or irrelevant as a tribe of Indians or John Smith's breakfast. The basis of the science is, then, firmly grounded in the uniqueness of particular events. To be precise, geology looks in two directions. In so far as it occupies itself with abstract masses and forces, it is a conceptual science, for which specific instances as such are irrelevant. In so far as it deals with particular features of the earth's surface, say a particular mountain chain, and aims to reconstruct the probable history of such features, it is not a conceptual science at all. In methodology, strange as this may seem at first blush, it is actually nearer, in this aspect, to the historical sciences. It is, in fact, a species of history, only the history moves entirely in the inorganic sphere. In practice, of course, geology is a mixed type of science, now primarily conceptual, now primarily descriptive of a selected chunk of reality. Between the data of the latter aspect and the concepts of the former lies that yawning abyss that must forever, in the very nature of things, divorce the real world of directly experienced phenomena from the ideal world of conceptual science.

Returning to social science, it is clear that the leap from psychology to social science is just of this nature. Any social datum is resolvable, at least theoretically, into psychological concepts. But just as little as the most accurate and complete mastery of physics and chemistry enables us to synthesize a science of geology, does an equivalent mastery of the conceptual science of psychology—which, by the way, nobody possesses or is likely to possess for a long time to come—enable us to synthesize the actual nature and development of social institutions or other historical data. These must be directly experienced and, as already pointed out, selected from the endless mass of human phenomena according to a principle of values. Historical science thus differs from natural science, either wholly or as regards relative emphasis, in its adherence to the real world of phenomena, not, like the latter, to the simplified and abstract world of ideal concepts. It strives to value the unique or individual, not the universal. "Individual" may naturally here mean any directly experienced entity or group of entities—the earth, France, the French language, the French Republic, the romantic movement in literature, Victor Hugo, the Iroquois Indians. some specific Iroquois clan, all Iroquois clans, all American Indian clans, all clans of primitive peoples. None of these terms, *as such*, has any relevancy in a purely conceptual world, whether organic or inorganic, physical or psychic. Properly speaking, "history" includes far more than what we ordinarily call historical or social science. The latter is merely the "historical" (in our wider sense), not conceptual, treatment of certain selected aspects of the psychic world of man.

Are not, then, such concepts as a clan, a language, a priesthood comparable in lack of individual connotation to the ideal concepts of natural science? Are not the laws applicable to these historical concepts as conceptually valid as those of natural science? Logically it is perhaps difficult, if not impossible, to make a distinction, as the same mental processes of observation, classification, inference, generalization, and so on, are brought into play. Philosophically, however, I believe the two types of concepts are utterly distinct. The social concepts are convenient summaries of a strictly limited range of phenomena, each element of which has real value. Relatively to the concept "clan" a particular clan of a specific Indian tribe has undeniable value as an historical entity. Relatively to the concept "crystal" a particular ruby in the jeweler's shop has no relevancy except by way of illustration. It has no intrinsic scientific value. Were all crystals existent at this moment suddenly disintegrated, the science of crystallography would still be valid, provided the physical and chemical forces that make possible the growth

of another crop of crystals remain in the world. Were all clans now existent annihilated, it is highly debatable, to say the least, whether the science of sociology, in so far as it occupied itself with clans, would have prognostic value. The difference between the two groups of concepts becomes particularly clear if we consider negative instances. If, out of one hundred clans, ninety-nine obeyed a certain sociological "law," we would justly flatter ourselves with having made a particularly neat and sweeping generalization; our "law" would have validity, even if we never succeeded in "explaining" the one exception. But if, out of one million selected experiments intended to test a physical law, 999,999 corroborated the law and one persistently refused to do so, after all disturbing factors had been eliminated, we would be driven to seek a new formulation of our law. There is something deeper involved here than relative accuracy. The social "law" is an abbreviation or formula for a finite number of evaluated phenomena, and rarely more than an approximately accurate formula at that; the natural "law" is a universally valid formulation of a regular sequence observable in an indefinitely large number of phenomena selected at random. With the multiplication of instances social "laws" become more and more blurred in outline, natural "laws" more and more rigid. However, the clarification of the sphere and concepts of social science in its more generalized aspects is a difficult problem that we can not fully discuss here.[1]

I strongly suspect that Dr. Kroeber will not find me to differ essentially from him in my conception of history. What I should like to emphasize, however, is that it is perfectly possible to hold this view of history without invoking the aid of a "superorganic." Moreover, had the uniqueness of historical phenomena been as consistently clear to him as he himself would require, it would be difficult to understand why he should have insisted on eliminating the individual in the narrow sense of the word.

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GEOLOGICAL SURVEY,
OTTAWA, CANADA.

THE AUTONOMY OF THE SOCIAL.

IN a stimulating disquisition on "The Superorganic" (this journal, April–June, 1917, pp. 163–214) Dr. Kroeber once more returns to the topic which of late he has made peculiarly his own.

[1] For a penetrating analysis of the fundamental distinction between historical and natural science I strongly urge all anthropologists, and social scientists generally, who are interested in method to refer to H. Rickert's difficult but masterly book on *Die Grenzen der naturwissenschaftlichen Begriffsbildung; eine Einleitung in die historischen Wissenschaften*. I have been greatly indebted to it.

With the major, critical, destructive part of the essay I must express unequivocal agreement. With Dr. Kroeber I hold that; (1) the biological point of view fails to account for social phenomena (autonomy of history with reference to race and heredity); (2) the psychological point of view fails in the same connection (autonomy of history with reference to general psychology); (3) the problem of individual ability is also irrelevant (autonomy of history with reference to individualities).

But my agreement with Dr. Kroeber stops short of his formulation of the concept of historic autonomy. Having rejected alternative interpretations, Dr. Kroeber voices his belief in cultural or civilizational determinism. Events in history occur when they must occur. As evidence, the author adduces the many instances in which similar, almost identical, inventions or discoveries were made contemporaneously and independently by two or more men. Here we must call for a more precise determination of the group the civilization of which is being considered. Is it the civilization of the world at large? If that were the case, then, of course, there would be truth in the proposition: What is must be, all that is is reasonable, in the sense of having a reason, a cause. Unless indeed we are prepared to admit that one or another part of the cosmic whole may suddenly run amuck and play havoc with law and order, such a universal determinism must, of course, be accepted as a conceptual necessity, with reference to civilization, or anything else. But in the solution of concrete historic problems such a deterministic faith, while basic, is helpful not at all. I believe, however, that Dr. Kroeber is not speaking of civilization but of civilizations. Now, with reference to any particular civilization the deterministic creed breaks down. Mass phenomena and the theory of probability will, of course, within their domain, continue to apply. An invention being a function of knowledge and effort, when the requisite state of knowledge is reached and a number of minds are concentrating on a certain problem, the probability of the invention or discovery being made by one or more minds is high, and if the number of minds is large, the probability will approach certainty. On the other hand, the same invention may be made at a time when the probability of that event is very low, owing to the presence of an exceptional mind or through importation from another civilization. An invention thus made may have the same or greater historic consequences than would be the case in the alternative instance. Now in this as in a thousand and one similar instances the events may not be regarded as civilizationally or socially predetermined; with reference to the culture of the group they are accidental. The theory of

probability breaks down here, the individual event alone counts; and it may make the difference, for all time, between a high and a low civilization.

My second point of difference with Dr. Kroeber refers to the relation of a civilization to the chain of individuals who are its carriers. I grant that the civilizational stream may be considered as a closed system and that much can be gained from studying it as such. Dr. Kroeber, in his turn, grants that useful knowledge, even about the stream of civilization, may be gleaned from a study of its component individuals. But this is not going far enough. As a matter of fact, the civilizational stream is not merely carried but is also unrelentingly fed by its component individuals. The term "individual," in this context, requires specification. It is not the biological individual, nor is it the abstract being of general psychology, nor is it this or that more or less gifted individual; it is not even the average individual who partakes of the given civilization. The individual who counts here may be designated as the biographical individual. He is a historic complex *sui generis*. Neither *biological* nor psychological, nor civilizational factors exhaust his content. He has partaken of the culture of his social environment, but only of certain aspects of it, and these have come to him in a certain individual order, at certain definite places and times, and have been, on all these occasions, received and absorbed by a psyche that was unique. This is the concrete individual of historic society. He is unique and as such he reacts on the civilization of which he is the carrier. Leave him out, and a blind spot appears in the record of civilization. I am convinced that not along the objective study of civilization lies that "path of merely but deeply understanding phenomena," to which the author refers at another place. The inferiority of our knowledge of Kwakiutl or Yoruba civilization when compared to our knowledge of the civilization of France or Germany is in part due to the inferiority of the civilizational record; but it is also due, perhaps more significantly, to the vast inferiority of the biographical record. This is the crying need the modern ethnologist tries to supply; and to the extent to which he is successful, our knowledge of primitive civilization becomes more deep.

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THE STATUS OF WASHO

MR. JOHN P. HARRINGTON'S announcement of the genetic relationship of Washo and Chumash (*American Anthropologist* (N.S.), 1917, p. 154) is welcome confirmation of a result which the undersigned had

independently arrived at, namely, the necessity of including Washo in the Hokan group established by Drs. Dixon and Kroeber. Specific Chumash-Washo resemblances had also been noted by the writer. That Chumash is a member of the Hokan group had been suspected by Drs. Dixon and Kroeber, further corroborated by incidental data advanced by the writer of this note in linguistic papers just published or soon to be published, and clinched by Mr. Harrington's previous announcement of the clear genetic relationship of Chumash and Yuman, a typical Hokan group of dialects. As for Washo, the writer has already gathered a quite considerable mass of lexical, phonologic, and morphologic data that, at least in his opinion, conclusively establish the Hokan character of this language. In grammatical respects, indeed, Washo would seem to be more typical of Hokan than Yana, which clearly belongs to the group (a paper on the status of Yana has recently appeared in the *University of California Publications in American Archaeology and Ethnology*). The writer hopes to reduce his Washo data to the form of a systematic paper before long. It is highly gratifying to note that far-reaching reclassifications of American languages are being independently and corroboratingly arrived at by students of American linguistics. Evidently the cautions that have been urged by certain more conservative students are not, despite their methodological excellence, exercising an unduly deterrent influence.

The Hokan stock can at present be stated to include Shasta-Achomawi, Chimariko, Karok, Pomo, Yana, Esselen, Salinan, Chumash, Yuman, Washo, Seri, and Chontal. A paper of the writer's, written some time ago, but publication of which has been delayed, undertakes to demonstrate the genetic relationship of the Hokan languages to the Western Gulf or Coahuiltecan group (Coahuilteco, Comecrudo, Cotoname, Karankawa, Tonkawa, possibly also Attacapan) recently set up by Dr. Swanton. It is very interesting to note that the territory separating Coahuilteco-Karankawa-Tonkawa from Yuman-Chumash-Washo is almost entirely occupied by Shoshonean and Athabaskan tribes, the latter of which are beyond all reasonable doubt an intrusive element from the north, while the former are only less certainly representatives of a relatively recent northward and eastward spread of Uto-Aztekan tribes from, say, southern Arizona and northern Mexico. The elimination of Washo leaves Yuki as the only Californian language that can be called "isolated." It is hardly to be expected that this privilege can be allowed Yuki indefinitely.

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GEOLOGICAL SURVEY,
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PROCEEDINGS OF THE AMERICAN ETHNOLOGICAL SOCIETY, INC.

Meeting of February 28, 1917

THE Society held a Tylor Memorial Meeting in honor of Sir Edward Burnett Tylor (d. Jan. 2, 1917). Dr. Goddard was in the chair, and the following members were present: Mrs. Donohugh, Dr. Fishberg, Dr. Frachtenberg, Dr. Goldenweiser, Dr. Haeberlin, Dr. Lowie, Miss Mallory, Miss Mayer, Mr. Mead, Mr. Morris, Mr. Nelson, Mrs. Parsons, Miss Roberts, Mr. Schleiter, Mr. Sullivan, Mr. Spier, Dr. Spinden, Dr. Wissler.

In the absence of Professor Boas, who was to have spoken on "Tylor's Influence," Dr. Wissler made some introductory remarks on the significance of Tylor in the history of anthropology and as a protagonist of evolutionary principles. From this he turned to the special topic of his discourse, "Tylor and Material Culture." While this phase of Tylor's activity seems relatively unimportant as compared with his other contributions, it must not be underestimated. Thus, it was he who blocked out the essential processes of primitive fire-making in his *Researches into the Early History of Mankind*, the later work of others being hardly more than an amplification, especially as regards facts of distribution, of his pioneer work. His discussion of cooking was equally important, and in the chapter on the Stone Age he clearly developed the point that the stone age represents rather a state of culture than a chronological epoch.

Dr. Lowie spoke next on "Tylor on Diffusion and Adhesions." While Tylor is generally associated with evolutionary schemes, it is interesting to find that he went rather far in explaining similarities by diffusion and in fact went farther in specific instances than most ethnologists would be inclined to go today, *e. g.*, in his discussion of the cure of sick people by sucking out the pathogenic agent. His treatment of the Malagasy bellows is a noteworthy instance of a positive contribution to ethnology by use of the principle of historical connection. The method of adhesions proposed by Tylor in 1888 and sketched the following year in the *Journal of the Royal Anthropological Institute* involved the application of a rigorous statistical treatment to ethnographic data. When two phenomena were found in conjunction with a certain frequency,

Tylor inquired whether this frequency was to be expected on the laws of probability, and if it was decidedly greater he assumed a causal relation between the two. The basic weakness of his discussion, as pointed out by Galton, lay in the neglect of diffusion: Tylor failed to consider in how far the several instances of correlation found might have spread from a single center. When, however, due attention is paid to the historical distinctness of the areas concerned, his method becomes an exceedingly valuable tool for ethnological research.

Dr. Goldenweiser then spoke on "The Doctrine of Animism." He explained Tylor's minimum definition of religion as the belief in spirits and pointed out that Tylor's treatment of the subject was rather a contribution to comparative religion than merely an evolutionary scheme. The speaker contrasted it favorably with Herbert Spencer's corresponding theory and again called attention to Tylor's recognition of the diffusion principle. Recent criticisms passed on Tylor's theory of animism come from several directions. The sociological school of France criticized the representation of religious phenomena from the viewpoint of individual psychology. Lang emphasized abnormal psychological phenomena over and above the normal ones on which Tylor laid the main stress. Further, Lang agreed with Father Schmidt in urging that primitive man already had a belief in a supreme otiose high-god. Again, Marett and others have insisted on the importance of the religious thrill, which is neglected in the more rationalistic view attributed to savage believers in spirits. Nevertheless, in its essential features Tylor's doctrine of animism still stands in spite of all modifications and amplifications of detail.

The president called on those present to open the discussion. Mr. Spier asked Dr. Lowie to explain how Tylor's method was applicable where the units were less clearly defined than, say, in the case of the mother-in-law taboo. Dr. Lowie replied that it was not so important whether the elements descriptively separated were historically distinct. In either case the establishment of a nexus would prove enlightening.

Dr. Frachtenberg expressed his appreciation of Tylor's religious theory and the confirmation he had found of it in the field among Northwestern tribes.

Dr. Lowie called attention to the fact that Tylor had had some field experience in Mexico in his early manhood and asked Dr. Spinden to make some remarks on Tylor's book *Anahuac*, now rare and little known in America.

Dr. Spinden explained that Tylor's work was a precocious effort still referred to for its descriptions of Mexican antiquities in British

collections. He added some remarks expressing the admiration experienced when first becoming acquainted with Tylor's *Primitive Culture.*

Dr. Goddard called attention to Tylor's promotion of the important field work in British Columbia undertaken by Dr. Boas for the British Association for the Advancement of Science and the consequent indebtedness to Tylor of all American students of ethnology. He suggested the publication of the addresses delivered this evening, as well as of Professor Boas' scheduled remarks.

The meeting adjourned at 10 p. m.

Meeting of March 26, 1917

THE meeting was called to order by Dr. R. H. Lowie, but Dr. Goddard, who had been delayed, took the chair on his arrival.

Mr. Herbert Lang, the African explorer, spoke first on "Dances of the Natives of the Belgian Congo." The tribes, whose dances were shown in a series of colored slides, are all resident in the northeastern Congo basin, covering an area which extends from the equator to nearly 10° north and from 1,200 to 2,000 miles inland. They are the Mangbetu, Medje, Makere, Abarambo, Azande, Logo and also the Pygmies. Their present distribution was explained by means of a map and remarks were made on the general influence of the physiographical conditions—especially the flora—on the population in respect to their social organization and their dances.

The dances considered may be divided into three groups: (1) those of a merely social order, *i. e.*, for amusement or the propagation of good fellowship; (2) ceremonial dances, which show great variety but are wholly adapted to circumstances; (3) the dances of a rather religious aspect for the propitiation of the bad spirits, for the sake of divination or similar reasons.

Slides illustrating the environment, the villages, and a few types of men and women led up to accounts of the usual preparations preceding each particular dance. Short discussions were presented of their costumes, hairdresses, the decorative designs on their body, and some of the musical instruments used, as well as the intoxicants that often provide the necessary feeling of gaiety or excitement.

The dances shown furnish an important contribution toward the better understanding of the psychology of these negroes. Immorality is practically barred and by far the greater number of all the dances are joyous demonstrations in which personal ability and individuality have

an opportunity to gain admiration or render themselves useful to the community. The craze for sensation is demonstrated in many ways by the costumes, as well as by the peculiarities of the dances. But foremost is the fundamental knowledge that, through easy abandonment to social enjoyments, they are able to submerge the trivialities and hardships of the daily life.

Dr. Elsie Clews Parsons next presented a paper on "The Office of Governor at Zuñi."

There are at Zuñi a governor (*tapup*) a so-called lieutenant governor (*sipoloa shiwanni*, Mexican rain-priest) and nine *tenientes*, a board of eleven only recently enlarged from eight. These officers are chosen by the six head *ashiwanni* (rain-priests). They hold office at the pleasure of the *ashiwanni;* but when they are to be relieved the change is made either at the Saint's dance in September or some time about January first. This year the *tenientes* who had been in office for five years were relieved and on January 16 new officers were installed. The ceremony, which consisted of passing the canes of office from outgoing to incoming officers was held in the house of the head rain-priest. It was managed by the head bow-priest. It was characterized by Mexican (Spanish) features—genuflexion and the sign of the cross. The use of physical force as a sanction, as mentioned in the governor's prayer, may also point to Spanish influence. The texts of the prayers said by the outgoing governor and the head rain-priest were subsequently secured and translated. They are the first prayers recorded, as far as I know, in Zuñi. The prominence of the *tenientes* at the celebration of the Saint's dance (they sit at the altar) is another indication of the Spanish character of their office. Spanish or not, the institution is thoroughly at home. Its undoubtedly Zuñi features are the long-time customary number of the *tenientes*, eight as a multiple of four being a favored Zuñi numeral, the control of the *tenientes* by the priesthood, the rite of breathing on the cane of office at the installation (the word *tapup* means breather on the cane), the fetichistic character of the cane (unless a *teniente* were in actual possession of his cane, his authority would be questioned), the obligation upon the *tenientes* to present a case or issue an order *four* times, and in some cases the crying out of orders *four* days in advance.

At Acoma corroboratory evidence of the Spanish origin of the institution was secured. At Acoma the governor (*tapup*) and two *tenientes* are chosen annually by the *cacique*, the head of the Antelope clan, the ruling clan. The installation occurs December 30, at the conclusion of a ceremonial week of church-going and of dancing. The

installation occurs in an assembly building called *komanira*. It is a house near the church and it is of Spanish rather than Pueblo architecture. In it the governor holds court. At the installation, genuflexion occurs as well as the removal of hats and the sign of the cross with the "Mexican prayer."

Some time after the installation (this year January 27 and when I was at Acoma) the *tenientes* and the three "war chiefs" elected with them give a "dance," the *hoinawe*. It corresponds to the *owinahaiye* dance of Zuñi, a dance which was associated in Zuñi with the bow priests, and which has not been danced for six or seven years. Firing off guns, a Spanish feature, used to figure in the Acoma dance as in the Zuñi. Throwing presents of food, etc., either to the dancers or by the dancers is still a feature in the *hoinawe* as it was in the *owinahaiye*. (At Zuñi *largess* seemed to have been originally confined to the *owinahaiye*, to the other bow-priest ceremonial, the scalp dance, and to the Saint's dance.) Largess in this form may have been introduced by the Spanish. The older custom in Zuñi was house-to-house collecting or sending out food to the ceremonial house.

Both at Zuñi and Acoma the bow priests or war-chiefs figure conspicuously, not only in the dances referred to, but in connection with the installation of *tenientes*. It is suggested that in pre-Spanish days the war chiefs (or bow-priests) were the secular arm of the priesthood, representing the kingly rather than the priestly power. With the advent of the Spaniards and the imposition by them of the type of government familiar to them and congenial, the war-chiefs tended to be deprived of their more secular functions and to be thrown back on their more sacerdotal functions. (At Laguna where the rain priesthood is disintegrated it is the war-chiefs who perform the monthly prayer plume plantings for rain and social welfare.) At the same time there grew up a tendency, more among the Keres than among the Zuñi, to connect the war-chiefs with the *tenientes*.

This hypothesis would help account for the influence of the war officers at Zuñi in selecting *tenientes*, for their function as masters of ceremony at the installation of *tenientes* alike at Acoma and at Zuñi, and for the performance of the *hoinawe* at Acoma to celebrate the *festa del re*.

At Acoma the war-chiefs became more assimilated with the *tenientes*, at Zuñi with the priesthood. At Acoma (and Laguna) the war-chiefs have survived through their assimilation with the *tenientes*. For lack of that assimilation at Zuñi there is a fair prospect of their disappearance.

It is an illustration of how the life of an institution may depend on being adapted to new circumstances.

The paper was discussed by Dr. Lowie, Mr. Morris and Mr. Spier.

Meeting of April 30, 1917

THE meeting was called to order at 8.25 p. m. Dr. P. E. Goddard presided. He welcomed the lecturer of the evening, Professor S. A. Smith, of the University of Sydney, who presented a paper, illustrated with slides, photographs, diagrams and a cast, on "The Talgai Skull; a Fossil Human Skull found in Queensland, Australia."

The lecturer explained that the skull had been found some thirty years ago, but had only recently been examined by scientific men. His discussion was necessarily a purely anatomical one. The skull belonged to a boy from fourteen to sixteen years of age. Its facial characteristics and general cranial aspect do not differ from those of normal Australian skulls, but the palate and dentition are of an exceedingly primitive character and on this may be based an argument for the very respectable antiquity of the Talgai man.

The paper was discussed by Dr. Wm. K. Gregory, Dr. J. H. McGregor, Dr. Milo Hellman, Mr. Louis R. Sullivan and Mr. Ralph W. Linton.

ROBERT H. LOWIE
Secretary

ANTHROPOLOGICAL NOTES.

Dr. John R. Swanton, of the Bureau of American Ethnology, returned to Washington in the latter part of June from an expedition to Louisiana, covering six weeks, during which he visited the remnants of the Houma, Chitimacha, and Koasati Indians. Some ethnological and linguistic notes were obtained from the two first mentioned, and a great many pages of texts secured from the Koasati. Koasati, although closely related to Alabama, differs from it somewhat more than had been supposed.

Mr. J. N. B. Hewitt, of the Bureau of American Ethnology, has returned to Washington after a field trip covering four months and devoted mainly to researches concerning the origin and structure of the League of the Iroquois. He spent most of his time on the Grand River Reservation, Ontario, but was present at the condolence and installation ceremony of the League which took place at Muncie, during the period of his visit.

Dr. Truman Michelson, of the Bureau of American Ethnology, left Washington on July 11, to resume his studies of the Algonquian tribes of Iowa, Kansas, and Oklahoma.

Dr. Aleš Hrdlička, curator of physical anthropology, U. S. National Museum, left Tuesday, July 31, for a trip among the Tennessee mountaineers, in connection with his work on old Americans. Dr. Hrdlička will be accompanied by Mr. King of Bristol, Tennessee, who knows the region and is acquainted among the mountaineers. From there Dr. Hrdlička will go among the Shawnee Indians of Oklahoma to examine the full-bloods still existing in that tribe, with a view of finally deciding the question as to whether the Shawnee are descendants of the important "stone grave" people of Kentucky and Tennessee.

Dr. Walter Hough, of the U. S. National Museum, continued work during the month of June on the ancient pit-dweller village at Luna, New Mexico, collecting additional artifacts and information. Another site containing fifteen acres was discovered seven miles north of the Luna site. A report on this work is in course of preparation. He also made observations on the ceremonial mask-headdress of the White Mountain Apache of Arizona, and collected a number of interesting specimens of paraphernalia.

Mr. Neil M. Judd, of the U. S. National Museum, was engaged during the Spring in effecting repairs on Batatakin ruin, Navajo National Monument, northeastern Arizona, for the Department of the Interior. Subsequently he took charge of archaeological work in Utah, which is carried on in cooperation by the Smithsonian Institution and the University of Utah.

THE excavation of the Aztec ruin in New Mexico, begun by the American Museum of Natural History last summer, is being continued on an extensive scale under the immediate direction of Earl H. Morris. The work has, so far, yielded a very fair return in specimens of unusual interest, particularly pottery and textiles.

Mr. N. C. Nelson left New York August 13, to round out the archaeological work in which he has been engaged for the past five or six seasons. This work had for its center the region of the Galisteo valley, but has spread to considerable distances in other directions.

THE degree of Doctor of Philosophy has been given by Harvard University to William H. Mechling, Carl E. Guthe, and N. Utsurikawa. The subjects of their theses were, respectively, *The Malecites and the Micmacs*, *The Luna Count of the Mayas*, and *The Art of the Malay Peninsula*. Mr. Mechling has received an appointment on the staff of the Field Museum and Mr. Guthe in the Department of Archaeology at Andover. Mr. Utsurukawa has received a Sheldon Traveling Fellowship and is to continue his studies in Malaysia.

During the temporary cessation of work at the Pecos ruin, Dr. Guthe of the Andover expedition staff, is carrying on explorations to the east of Pecos and in the northern Rio Grande, in an endeavor to define the eastern and northern limits of Pueblo culture in New Mexico and Colorado.

THE Natural History Collections of the Peabody Museum of Yale University, including the Anthropological Collections, have for the most part been stored pending the erection of a new museum building. Meanwhile a few of the anthropological exhibits, including the Moseley collection of Indian basketry and the Prudden collection of antiquities from the Southwest have been installed in Memorial Hall.

WILLIAM WALLACE TOOKER, an authority on Indian nomenclature and archaeology, died on August 1, after a long illness at his home in Sag Harbor, L. I., at the age of sixty-nine years.—*Science*